MISCELLANEOUS

EXPERIMENTS AND REMARKS

ON

ELECTRICITY,

THE

AIR-PUMP,

AND THE

BAROMETER:

WITH THE DESCRIPTION OF AN

ELECTROMETER OF A NEW CONSTRUCTION:

ILLUSTRATED WITH COPPER-PLATES AND NOTES.

By A. BROOK, of NORWICH.

Or by colliſion of two bodies grind
The air attrite to fire, as late the clouds
Juſtling or puſhed with winds, rude in their ſhock,
Tine the ſlant lightning, whoſe thwart flame driv'n down
Kindles the gummy bark of fir.

SECOND EDITION.

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THE Electrometer mentioned in the title-page has been publiſhed in the Philoſophical Tranſactions, Vol. LXXII, but by ſome means not generally known, and the omiſſion there of many neceſſary references (although the ſame drawings of the plates were in the hands of the compilers of that very uſeful and extenſive publication) it is not eaſy to diſcover the mechaniſm, nor the method of uſing the inſtrument; from which it was thought, that a republication, and a more accurate deſcription thereof, might make it better underſtood, ſo that the workmanſhip may be more eaſily executed, and the inſtrument be uſed without difficulty: and if the language, in which the following ſheets are written, can be underſtood, the Author will obtain what he aimed at, having no great pretenſions to erudition, and meaning only to relate plain facts.

NORWICH,
1786.

INTRODUCTION.

AS ſo many excellent pieces, by way of introduction to electricity, have already been written, what is contained in the following ſheets is not ſo much intended for the inſtruction of beginners in that branch of natural philoſophy, as to find out ſome way of admeaſurement thereof, ſo as to be generally underſtood; or in ſuch a manner, that one perſon may communicate experiments to another, at any diſtance of time or place, by ſuch a meaſure, as that they may both ſucceed alike. Accordingly, after having deſcribed the electrometer, &c. repreſented by the copper-plates, by which ſuch an admeaſurement is to be aſcertained, a courſe of experiments is ſet down in the promiſcuous order they were made. The former part of them, to about Experiment XXIX. were trials with different numbers of bottles, charged to different heights, on wires of different metals, ſizes, and lengths, in order to find out the ſtrength of the charge of a given quantity of coated glaſs neceſſary to produce the effect propoſed. The latter, are experiments, where thoſe charges are reduced more to a certainty, either with poſitive or negative electricity, ſo as to know, before-hand, what ſtrength of charge is neceſſary to melt a wire of either lead, iron, braſs, or copper, of a given length and ſize; which will tend to point out pretty nearly (where bell-wires have been melted by a thunder ſtroke, the ſize, ſort of metal, and the length of the wires that were deſtroyed being known) the ſtrength of the ſtroke from the cloud where ſuch accidents have happened; and thence to find what quantity of coated ſurface is neceſſary, and to what height it muſt be charged, in order to do the ſame.

The battery with which ſome of the following experiments were made, conſiſted at firſt of only nine jars, or rather bottles, for they were not with open mouths; but when a greater power was wanted, it was neceſſary to procure more bottles, or jars; but the latter not being eaſily procured, the former were ſubſtituted in lieu of them; though afterwards a much leſs number of bottles was uſed, ſolely for trial ſake. The mean ſize of theſe nine bottles was eight inches diameter, coated about ten inches high, of the thickeſt and ſtrongeſt glaſs that could be procured*, weighing from five and a half, to about ſeven and a half pounds each. The bottles which compoſed the battery after Exp. XXXII, were diſpoſed in three rows, nine in each row; nine of the ſtouteſt and beſt compoſed the firſt row, nine of the next in ſtoutneſs made the ſecond row, and the third row contained the nine weakeſt, (of which notice will be taken in the work) all of green glaſs, but not all of one ſort of green glaſs. Some of them, which ſtood foremoſt in the battery, were made of a ſort of glaſs very much like that of the Frontiniac wine bottles. The glaſs of which thoſe bottles are made ſeems to be by much the beſt, being much harder and ſtronger, and ſeems to bear the higheſt charge without being ſtruck through. One of theſe wine bottles which is three inches and a half diameter, and coated ſeven inches high, has often been charged to thirty grains repulſion (between two balls of the ſize of thoſe on this new conſtructed electrometer) and has not been ſtruck through.

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* See Remark X.

The ſecond and third rows in the battery, conſiſted of bottles from about ſix and a half, to ten inches diameter, coated from eight and a half to eleven inches high; and none of their mouths are more than one inch and a half, nor leſs than three quarters of an inch wide, in all the battery. The higheſt charges being made with bottles apparently the ſtrongeſt, if any of them were ſtruck through, the fracture was repaired as in Experiment XLIII, and removed to a lower order, when a deficiency happened there, as will be obſerved in the work: and when any meaſure of coated glaſs or ſurface is mentioned, it is meant only of external coating.

Notwithſtanding the above battery conſiſts of ſuch *narrow* mouthed bottles, on account of *wider* not being eaſily procured; yet, the latter are much preferable, being eaſier to coat; moreover, the electricity is not ſo liable to be diſperſed by the wires that go into the bottles when their mouths are wider; for if particular care be not taken, (and even this will hardly be ſufficient) the electricity will frequently be ſpurting from the ſides of the wire, to the edges of the mouths of the bottles, when they are thus narrow, without bringing on a ſpontaneous diſcharge. This being the caſe with the Frontiniac wine bottles, I have never been able to charge them ſo high as they would bear: therefore I cannot tell how much more, than to thirty grains, they might be charged, without being ſtruck through: but by the time that the charge gets to be thus high, the ſpurting will diſperſe the fluid as faſt as the machine will collect it. The number of bottles that have been deſtroyed in making the following experiments, has, in ſeveral of them, made a ſmall difference in the reſult; but could the ſame battery have been preſerved intire, throughout the making of theſe experiments, I believe there would have been ſo little difference, as not to be of any material conſequence, and ſtill leſs difference, if wires perfectly homogeneous, could have been procured. This difference appears moſt in the braſs wire (at the former part of the experiments, when my ſtock of braſs-wire was very low, and I was never able to procure any more like it) with regard to its ſtretching ſo much while hot, which, to me, appeared very ſingular.

As wires of exactly the ſame ſize may be rather difficult to procure at all times, and leſt a method to prove them ſo, may not be always at hand, the method I took to prove what thoſe wires were, both in weight and ſize, that I uſed in the following experiments, is here ſubjoined. Each piece of wire that was weighed was cut ten inches and a quarter long; as every piece of wire that I tried was the length ſpecified in the experiment between the forceps with which it was held. I have alſo added the weight of a piece of each wire, both of their ſizes, and the ſort of metal: every piece of wire that was weighed was the ſame in length. The weight of them is added to be a check upon the meaſure of their thickneſs; and the ſcales were ſuch as turned freely with the two hundred and fifty-ſixth part of a grain. In order to get the thickneſs of each wire, as near as poſſible, they were wound round a wire one-tenth of an inch thick, on which an inch ſpace was carefully marked; the revolutions of the wires being kept as cloſe as poſſible to each other and their number being known, gives the thickneſs of each wire, pretty nearly: the iron and braſs wire was ſuch as is uſed for harpſichords, &c.

All my battery, as well as all my other bottles, or jars, both withinſide and without, is coated with ſtrips, cut from three-eighths to three-fourths of an inch wide, and laid on with paſte made with flour and water, at the diſtance of about a ſtrip between each.

The following table exhibits the ſizes and weights of various wires of four different ſorts of metal.

Parts

	Parts of an Inch in thickneſs.	Weight of each in Grains and Parts of a Grain, $10\frac{1}{4}$ Inches long.	Proportion to each other.
Lead Wire.	66th	5 grains and $\frac{33}{64}$ of a grain	above half the 50th.
	56th	7 grains and $\frac{3}{16}$ of a grain	
	50th	9 grains and $\frac{1}{4}$ of a grain	nearly double the 66th.
Iron, or Steel Wire.	170th	$\frac{5}{8}$ of a grain	almoſt half that of 100th.
	140th	$\frac{45}{64}$ of a grain	
	120th	$\frac{31}{32}$ of a grain	
	100th	1 grain and $\frac{9}{32}$ of a grain	more than double 170th.
	90th	1 grain and $\frac{45}{64}$ of a grain	
	80th	2 grains and $\frac{11}{32}$ of a grain	almoſt double 100th.
	64th	3 grains and $\frac{37}{128}$ of a grain	
	56th	4 grains and $\frac{87}{128}$ of a grain	more than double 80th, or almoſt four times 100th.
Braſs Wire.	170th	$\frac{21}{32}$ of a grain	almoſt one-ſixth part of 50th.
	120th	1 grain and $\frac{1}{8}$ of a grain	
	64th	5 grains and $\frac{27}{64}$ of a grain	
	50th	6 grains and $\frac{83}{128}$ of a grain	more than ſix time 170th.
Copper Wire.	170th	$\frac{19}{32}$ of a grain	about $\frac{3}{5}$ of 140th, which is equal againſt the electrical ſtroke as 100th of ſteel wire, as appears by the following experiments.
	140th	$\frac{109}{128}$ of a grain	
	130th	$\frac{119}{128}$ of a grain	

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ERRATA.

PAGE 36, line 6, read *nearly* inſtantaneouſly.

—— 86, laſt line but one, read *or* as in.

—— 89, line 15, for 63, read 81 and 82.

Plate. 1.

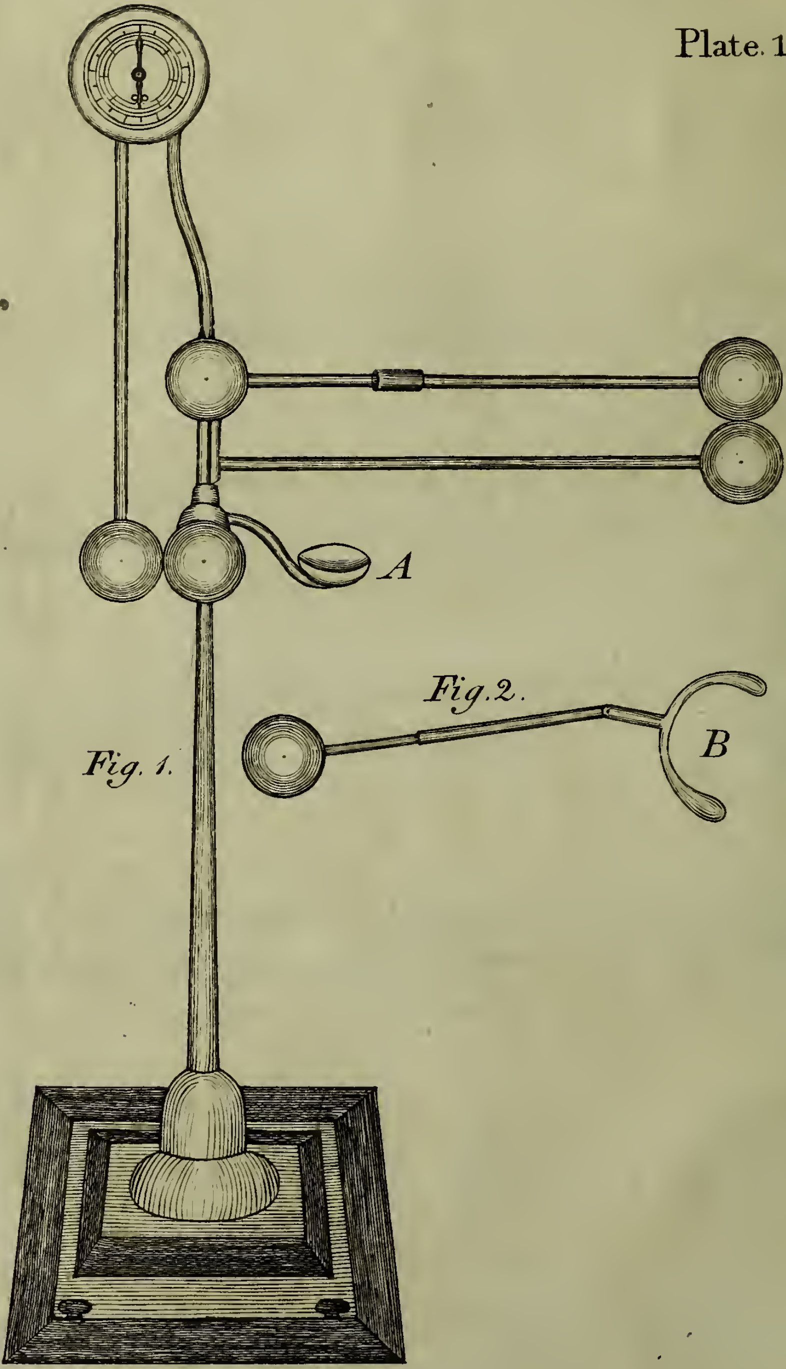

Fig. 1.

Fig. 2.

CHAPTER I.

DESCRIPTION

OF THE

PLATES.

AS an accurate admeaſurement of quantities of Electricity is amongſt the various deſiderata in that branch of natural philoſophy, and as the Electrometers which have heretofore been invented, have, in that particular, been found deficient; it is not without the hope, in ſome degree, of ſupplying that deficiency, particularly in large quantities, that the following explanation of a new inſtrument of this kind is offered to the public.

Plate 1, fig. 1, ſhews the Electrometer in miniature, as it appears when it is ready to be uſed. Fig. 2, is an arm, the ball of which is to be laid into the cup *A*, to make a communication to a battery, &c. to be explained afterward.

Plate 2, exhibits the outlines of all the different parts, both internal and external, of the Electrometer, in full ſize and proportion, and the manner in which they are made and put together.

A, *A*, *A*, *A*, *N*, fig. 1, and fig. 1, *a*, repreſent the Electrometer in full ſize all put together; except where fig. 1, *a* is ſeparated from fig. 1.

The foot *B*, fig. 1, *a**, is a piece of board, nine inches and three quarters ſquare, reſting on three pins *C C c*, ſeen at the under ſide of it. The pins *C C*, with their broad heads, are ſcrews to ſet the Electrometer upright withal. *D*, is a ſolid piece of glaſs, of any length and ſize, ſufficient to ſupport and inſulate the inſtrument, wherever it is to be placed: the length of that here deſcribed is about nine inches and a half between the ſockets in which it is fixed, and its upper end is ſeen in the ſocket, or cap *M*, fig. 1.

---

* This figure is wholly omitted in the Philoſophical Tranſactions, although it is referred to.

The arms *G* 1, and *G* 2, and the balls *I* 1, and *I* 2, fig. 1, are all made hollow, of thin copper, that they may be as light as poſſible. The arms *G* 1, and *g*, with the ball *F*, turn round on the large bent wire *H*; and when in uſe they are put nearly at a right angle with *G* 2, and *H*; being all turned to the off ſide, ſo as to be, as much as poſſible, out of each other's atmoſpheres, or the atmoſphere of a jar, battery, prime conductor, or the like. At *K*, is repreſented a kind of face, or dial plate, with an index; this index is carried once round by the motion of the arm *G* 2, with its ball *I* 2, moving through a quarter, or ninety degrees of a circle; the motion is given to the arm and its ball, by the repulſive power of the charge of a jar, &c. between the two balls *I* 2, and *L*. The ends of the index, from the center, are of different lengths; the longeſt end reaches to a graduated circle, divided into ninety equal parts, anſwering to the ninety degrees, which the arm *G* 2 moves through: the ſhorteſt end reaches to a ſmaller circle, which may be divided into any number of equal parts anſwering to the diviſions on the arm *G* 1, with its ſliding weight *n*, each of which, is equal to one grain, and the whole face is covered with a watch glaſs*, to prevent the electricity from flying off, or eſcaping at the points.

The upper end of the glaſs-ſupporter *D*, is cemented into the braſs cap, or ſocket *M*, fig. 1. This cap enters the ball *L*, at the bottom of it, and is ſcrewed into its upper part at *a*. The top part of the cap *M*, is tapered off to a cone about one inch and a half long. The lower end of the wire *H*, has a hole made conically into it, ſo as to receive the upper part, or conical end of the cap *M*, which permits all the upper part of the Electrometer to turn round any way that may be neceſſary. The kind of ferrel *O*, with its baſe, is perforated, that the lower end of the wire *H*, may go through it. The arm *b*, which ſupports the cup *N*, is ſcrewed into the baſe of the ferrel *O*, and turns round upon the wire *H*. The cup *N* is to receive the ball *P*, of the arm fig. 9. This arm, which is ſhewn ſeparate in plate 1, fig. 2, ſhortens or lengthens as may be wanted, by a wire ſliding into a tube; the end of the wire is ſlit, forming a ſpring in the tube, to give it ſteadineſs. In this arm is a kind of rule joint at *d*, that it may give way eaſily if wanted: the ſemicircular end of the arm is a ſpring, which may be ſlipt on to a ball from the prime conductor, or the conductor itſelf, (if it fit) or jar, or battery: the ends of it are flat and broad, as repreſented at *B*, fig. 2, plate 1.

---

* There was an Electrometer conſtructed by a very ingenious Gentleman ſome years ago, the face of which was covered with a glaſs: but it ſeemed to me to be leſs uſeful than Henley's Electrometer, though much more complex; yet its neatneſs was very pleaſing.

Fig. 2, ſhews the wheels &c. in the Electrometer*. The wheel *A*, has forty-eight teeth, and takes into the wheel *B*, of twelve teeth. The axis of *B*, carries the index *K*, fig. 1, once round, by the arm *C*, fig. 2, being repelled to *D*, a quarter, or ninety degrees of a circle, as before obſerved. The arm *C*, is ſcrewed into the underſide of the axis of the wheel of forty-eight teeth. *E*, is a ſolid leaden weight, ſupported and made faſt to the upper ſide of the ſame axis; this leaden weight, which is almoſt a counterbalance to the arm *G* 2, and its ball *I* 2, fig. 1, may be made of any ſhape, ſo that it be heavy enough, and move freely within the Electrometer. At *F*, fig. 2, is ſhewn a circular piece of braſs, fixed within ſide, ſo as that the repulſive power, at the top of the leaden weight *E*, may be as equal as poſſible, at whatever diſtance the arm *C* is repelled. At *G*, is ſhewn the piece of braſs into which the upper end of the wire *H*, fig. 1, is ſcrewed, it is made faſt to the inſide of the frame of the inſtrument with a ſcrew.

---

* All the references and deſcriptions of fig. 2, to fig. 11, are entirely left out in the Philoſophical Tranſactions, and only fig. 3, fig. 4, and fig. 10, are kept the full ſize, although otherwiſe expreſſed. All the other figures are much diminiſhed.

Fig. 3, ſhews the internal frame of the Electrometer, to which all the parts are fixed and ſupported by ſcrews, &c.

Fig. 4, ſhews the inſide of the Electrometer in profile. The braſs rim *A*, which holds the glaſs in the front face of the inſtrument, is kept in its place by center-pins at *c c*, and is held on by the ſcrews *a a*. The part *B*, which brings the dial plate forward from the frame towards the glaſs, is held on by the ſcrews *b b*. The axis of the wheel of twelve teeth paſſes through the dial plate on *B*, and receives the index *k*; the other end of the axis is pointed, and works in the bent arm *d*, which is ſcrewed to the frame at *e*. One end of the axis of the wheel of forty-eight teeth is pointed, and works in the frame; the other end is ſupported by, and works in, the bent arm *g*, which is ſcrewed to the frame at *h*; but to leſſen the friction at *i*, in the bent arm *g*, a ſcrew paſſes through it; this ſcrew is perforated, as at fig. 10, (repreſented enlarged) by this means, ſuch exactneſs in the length of the arm *g*, is unneceſſary, the ſcrew itſelf being perforated with a larger hole till very near the little end of it, which is bevelled off, and the remaining part of the hole being made very ſmall, the friction is conſiderably diminiſhed. The joinings of the weight *E*, and the arm *C*, are ſhewn here, the ſame as in fig. 2. The part *D*, which brings the dial plate on the back of the Electrometer towards the glaſs, reſts in the rabbet of the frame, and riſes half the depth of the rabbet. The braſs rim *F*, which holds the other glaſs, reſts in the ſame rabbet of the frame, fills up the other half of the rabbet, and is held on by a bent pin, as at *o*, on one ſide, and by a ſpring catch *p*, on the other ſide; this bent pin, and the ſpring catch paſs through two holes (under, or behind the braſs rim *F*,) made for that purpoſe in the part *D*. The axis of the wheel of forty-eight teeth paſſes through the dial plate *D*, and carries the index *m* a quarter, or ninety degrees of a circle, which may be graduated anſwerably to the face on the front of the Electrometer.

Fig. 5, repreſents the Electrometer turned its bottom upward, to ſhew the opening, where the arm *G* 2, fig. 1, moves up and down, and the hole through which goes the upper end of the wire *H*, fig. 1, and ſcrews into *G*, fig. 2. The catch *p*, which is ſhewn in fig. 4, is fixed to the frame, cloſe to the opening in fig. 5, that it may be opened eaſily with the point of an awl, or the like.

Fig. 6, fig. 7, fig. 8, fig. 11, ſhew the mechaniſm of the movements and parts in the ball *F*, fig. 1.

Fig. 8, ſhews the external conſtruction of the ball *F*, fig. 1, which is hollow, and conſiſts of a frame, or rim, in the middle, with two caps ſcrewed on to the rim, one on each ſide; it has a hole at top and bottom, through which goes the bent wire *H*, fig. 1. Within this ball is another leaden weight, which is a counterbalance to the arm *G* 1, fig. 1. The innermoſt circle in fig. 8, repreſents the outline of the weight in the ball *F*, fig. 1. The poſition of this weight is ſeen at *g*, fig. 6, and *g*, fig. 7; a hole is made perpendicularly through the middle of it, to give liberty for it to move up and down, free of the wire *H*, fig. 1, which paſſes through this weight.

Fig. 11, ſhews a piece of braſs, a little thicker than a new ſhilling, which is held to the inſide of the rim, fig. 8, by the ſcrews *a a*, fig. 6, with its hole in the middle, againſt the hole in the rim fig. 8, in which the arm *G* 1, fig. 1, moves a little way up and down, as ſhewn in fig. 6. The ends *b b*, fig. 11, are turned up as at *b b*, fig. 7, to form a ſupporter for the axis on which the arm *G* 1, fig. 1, with its weight *g*, fig. 7, reſts. *a a*, fig. 11, ſhew the holes, through which go the ſcrews *a a*, fig. 6. By theſe four figures the whole conſtruction is ſhewn better than by a whole page in letter-preſs. This weight is made faſt to the axis on which it is ſupported; but the arm *G* 1, fig. 1, ſcrews in and out as at *c* fig. 6.

Fig. 12, ſhews the part at *z*, fig. 1, that ſcrews into the ball *F*, to ſupport the arm *g*, and its ball *r*: this piece (which is hollowed out on the ſide next the wire *H*, to fit it) is ſcrewed in, ſo as to preſs againſt the wire *H*, and ſerves as a ſpring to keep the ball *F* ſteady; which is made to ſlide up and down, as well as to turn round on *H*, to whatever poſition may be wanted.

In order that the diviſions, before-mentioned, on the arm *G* 1, fig. 1, may be made exactly a grain each, firſt ſlide the weight *n*, towards the ball *F*, till it is an exact counterbalance to the weight in *F*; then at one end of the weight *n*, make a mark on the arm *G* 1, and there let the diviſions and numbering begin. Suſpend any tolerably good pair of ſcales, ſo as that the bottom of one of them may reſt upon the ball *r*, between *r* and *I* 1, (the ball *I* 1, reſting in the ſcale) ſlide the weight *n*, near to the ball *I* 1,

put

put as many grains into the other ſcale, as will juſt raiſe the ſcale with the ball *I* 1, in it; then mark the arm at the ſame end of the weight *n*, and divide the ſpace between the two marks, into as many equal parts as there are grains in the ſcale; each part will then be equal to one grain: each part may alſo be divided and ſubdivided into halves and quarters.

The arm *G* 2, fig. 1, being repelled, ſhews when the charge is increaſing, &c. and the arm *I* 1, with its weight *n*, tells what ſuch repulſive power is between two balls of the ſize of theſe, in grains, according to the number of diviſions, at which the weight *n*, on the arm *G* 1, reſts, when it is lifted up. The weight *n*, by repeated trials, having been put at different places, ſuch reſpective number of grains may then be marked on the leſſer circle on the dial-plate, to which the ſhorteſt end of the index *K*, fig. 1, reaches, and where it points at*: ſo

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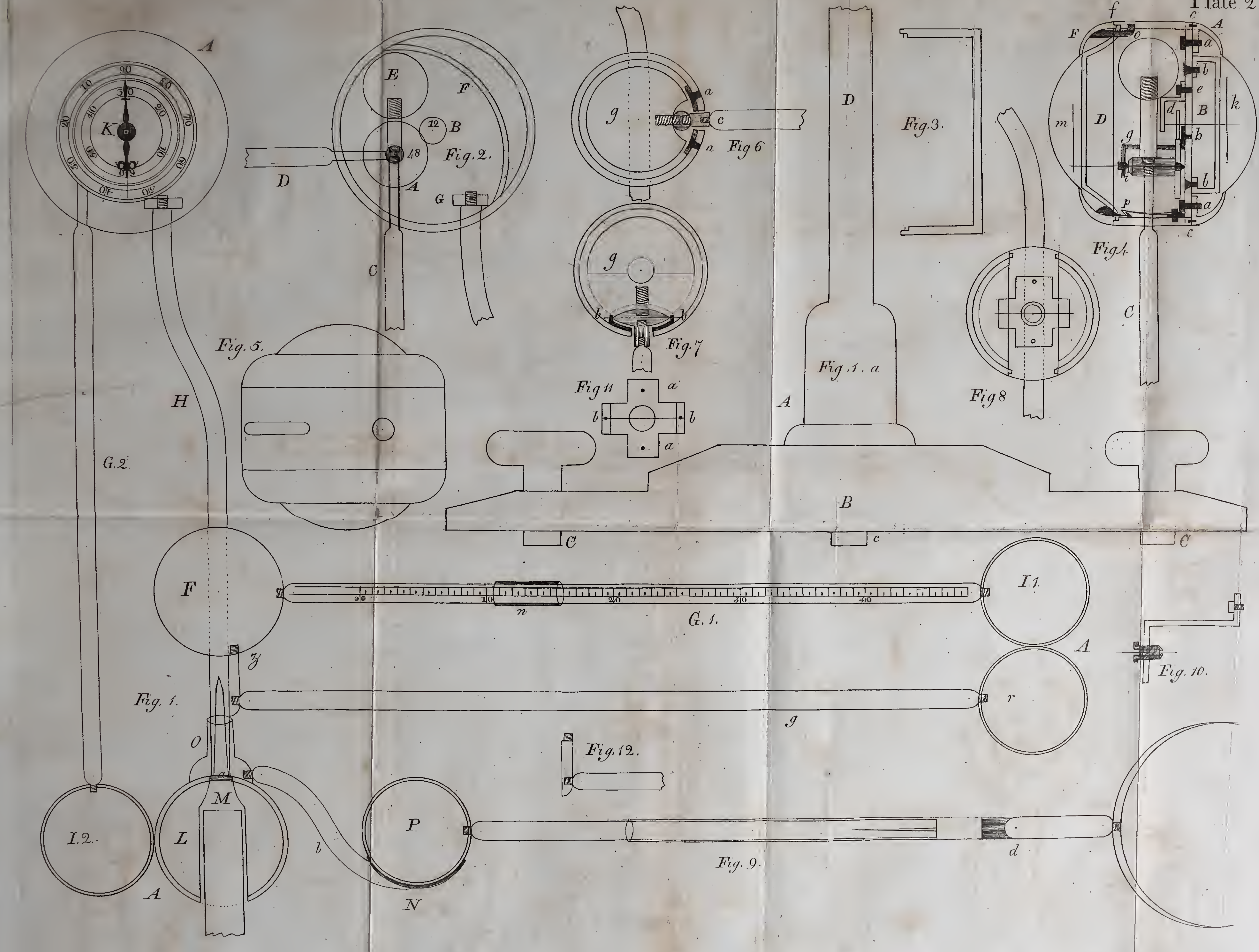
* The Gentleman who invented the Electrometer, mentioned in the foregoing note, conſtructed it ſo that the index was carried twice


A
K
90
10
80
20
70
30
40
50
60
D
E
F
12
B
48
A
C
G
Fig. 2.
g
a
c
a
Fig 6
g
b
b
Fig 7
Fig 11
a
b
b
a
D
Fig. 3.
Fig. 1. a
A
B
c
C
C
f
F
o
c
A
a
b
e
d
B
k
m
D
g
b
i
l
a
p
c
Fig 4
C
Fig 8
Fig. 5.
H
G.2
F
10
20
30
40
n
G.1
I.1
A
r
g
z
Fig. 1.
O
a
M
L
I.2
A
b
P
N
Fig. 12.
d
Fig. 9.
Fig. 10.

ſo that when the grains are all marked on the dial-plate, thus aſcertained by the arm *G* 1, all theſe parts of the inſtrument, that is, the ball *F*, with the arms *G* 1, and *g*, may be taken away, and the remaining part of the Electrometer may be uſed without them: but I do not know how the grains can be ſo exactly marked and aſcertained on the dial-plate, as by theſe parts being on the inſtrument. And, notwithſtanding theſe parts may be taken off, and the Electrometer uſed without them, yet I think it is moſt ſafe and certain, to uſe it with them altogether, as they are a check one upon the other: for I find various circumſtances will occur, by which

twice round the dial-plate, and he told me that a friend of his was going to make one with the index to go four times round, but I think it muſt be quite unneceſſary, and not ſo diſtinct: alſo, that the aſcertaining, and marking the grains on the dial-plate, as well as the number of degrees the moving arm is repelled, muſt be very indiſtinct. But his inſtrument (which was the firſt that I ever heard of, where the wheel-work was introduced) was not conſtructed ſo as, in any degree, to ſhew what the repulſive power was, any more than that of Mr. Henly's conſtruction. In order to have as much diſtinctneſs as poſſible, I made the index to go only once round the dial-plate, by which means both ends of the index become uſeful and intelligible.

the arm *G* 2, alone is more liable to vary at different times, than when the other parts are on: the atmoſpheres of different things, or the atmoſphere of the arm and its ball, interfering with other things, or any ſmall piece of lint, or other fibrous ſubſtances, lighting on the ball, will make it vary very much, according to the place it may, by chance, adhere to: but, as before obſerved, when they are all together they check each other. I believe the arm *G* 2, alone, would be equally certain, if all imperceptible atoms, &c. could be got rid of which are conſtantly floating about in the air of moſt rooms.

I do not mean to confine the number of grains, or diviſions on the arm *G* 1; but my own obſervations rather lead me to think, that no glaſs to be charged, as it is called, with electricity, will bear a greater charge than that whoſe repulſive force, between two balls of the ſize of theſe, is equal to ſixty grains weight, before it will be perforated, or ſtruck through. Nay, I have not found many inſtances when it would bear, or ſupport itſelf with ſifty grains; and I think it very hazardous to charge higher than ſorty-ſive grains. I think, likewiſe, that the ſize of theſe balls and wires are large enough to prevent the eſcape, or diſperſing of the charge of electricity, which any glaſs will bear, that we are acquainted with; but if balls, wires, or arms, of this ſize, are found too ſmall, larger may be made on the ſame plan.

After this manner, with the Electrometer above deſcribed, by knowing the quantity of coated ſurface, and the diameter of the balls, one may ſay, a certain quantity of coated ſurface of glaſs, charged to any given number of grains, expreſſing the repulſive force, between two balls of a certain diameter, will melt a wire of a certain ſize, kill this or that animal, and the like.

In reſpect to the uſes and advantages of this Electrometer, perhaps I may not know them all, and leſt partiality may prejudice me in behalf of my own contrivance, I would rather leave them to the judgment of others: my opinion however is, that all others, which I have ſeen or heard of, are ſuch as ſpeak no intelligible language, and that this ſpeaks ſo as to be underſtood univerſally: for unleſs the repulſive power of the charge of different glaſſes be very different, this Electrometer, or any other Electrometer, made after this manner, muſt, I ſhould preſume, ſpeak very nearly the ſame language; it being known how much coated ſurface there is, and the ſize of the balls: but if the ſize of the balls be not the ſame, the language the Electrometer ſpeaks, will be very different.

Although other Electrometers ſhewed a greater or leſs charge, or power, by an arm being repelled to a greater or leſs diſtance, or by ſtriking at different diſtances; yet, the power of the charge, was not in any manner aſcertained; we could ſay, that the arm, or the index, was repelled to ſuch a number of degrees of a circle, or that it ſtruck at a certain diſtance; but the repulſive power of a charge to repel the index ſo much, or ſo many degrees of a circle; or the ſtrength of the charge to ſtrike at ſuch a diſtance, was not, that I know of, in any manner intelligibly aſcertained: this does it by the weight that the repulſive power has to lift up in grains, &c. which weight is to be proved by any good pair of ſcales and weights; and I know not of any method that has yet been tried to ſhew the different ſtrength of charges, ſo ſatisfactory as by that of their repulſive force.

All the neceſſary parts of the Electrometer being made of metal and glaſs that is pretty ſtout, I think that the electricity is conſiderably leſs liable to eſcape than by wood, &c. I have tried reeds, on account of their being light, and covered them with tin-foil, or gilded them, to make them good conductors, but have ſo frequently found inconveniencies from them by points riſing up, the celerity of moving, and the different weight of them at different times, owing to moiſture, changes of the weather, and the like, that I have laid them all aſide, and find my preſent inſtrument as free from theſe inconveniencies as I could

expect;

expect; nor is it liable to be out of order, if proper care be taken of it.

Another uſe of this Electrometer is, to graduate other electrometers; I mean ſuch as Mr. Nairne's, which I think is a very good improvement of Mr. Lane's electrometer; and I think I have rather improved Mr. Nairne's, by a little alteration in the conſtruction of it, and graduating it, ſo as to be always certain; and it may be applied to any prime conductor, &c. but unleſs it be firſt graduated, that the ſtrength of the charge may be aſcertained by the diſtance it is to ſtrike at, it is of no more uſe than Mr. Nairne's; except that it is applicable to any thing, or almoſt any where: yet, after all this alteration, it will never ſhew when a charge is increaſing or decreaſing, nor how far off, or how near a diſcharge is, but only tell what it is, at the time that the diſcharge is made.

Although the electrometer deſcribed in plate 1, and plate 2, has the advantages and uſes already mentioned, yet, it will never make the diſcharge of a jar, or battery, of itſelf, for which Mr. Nairne's is very excellent: and in the ſtate to which I have altered it, I think it is exceedingly uſeful with the former one; thus, the progreſs of a charge is ſhewn by the motion of the arm *G* 2, of the former; the diſtance it is repelled ſhews how far off, or how near, a diſcharge, of any required ſtrength is; the arm *G* 1, will ſhew that that repulſive power is what it was deſigned to be; and the latter, will make the diſcharge, as ſoon as the charge has acquired the ſtrength propoſed: which I have proved, having one to apply to the machine that was uſed in making the following experiments.

Plate 3, fig. 4, ſhews this laſt mentioned electrometer in its improved ſtate. *A*, is a ſolid piece of glaſs bent to a right angle at both ends; one end of it goes into a ſocket cut in the ball *B*, which is of ſolid metal, to one ſide of which is made faſt a ſhort conical tube of tin. The prime conductor has likewiſe a conical tube, or ſocket, fixed in

the

the ſide or end of it, to receive the conical tube fixed to the ball *B*: they are made conical that they may ſtand ſteady. The other end of the glaſs *A*, goes into the ſocket of wood *C*, at the end of which is a ball; through this ball goes the graduated wire *D*, one end of which terminates in a ring or with a ball: the other end has another ball on it, equal in ſize to the ball *B*, both of which are the ſize of the balls of the electrometer deſcribed in plates 1 and 2. The ſocket cut in the ball *B*, being concentric to the ball itſelf, the glaſs *A*, will turn about in it; ſtill the ball on the graduated wire *D*, will always be at the ſame diſtance from the ball *B*.

Notwithſtanding all the improvements and alterations that have been made on Mr. Lane's electrometer, both as to acquiring charges of exactly the ſame ſtrength with the ſame quantity of coated ſurface, and as anſwering the end, or uſe, of a diſcharging rod, ſtill, with reſpect to the latter, it is chiefly uſeful, when the charges are not very high, nor a great quantity of coated ſurface; many fibrous particles (conſtantly and almoſt imperceptibly floating in the air of a room) being ſo apt to be attracted by, and adhere between, the balls of the electrometer; and the electric atmoſphere extending, ſo ſtrongly, much farther than the diſtance of theſe two balls; ſo that, in both theſe caſes, the charge is very liable to be diminiſhed, almoſt imperceptibly, whilſt the battery is charging. Therefore, with large batteries, highly charged, a diſcharging rod, or a contrivance for that purpoſe, is much better by itſelf, entirely detached from the machine, or the prime conductor; and in ſuch a manner, that it may always approach the place, where the diſcharge is to be made, with the ſame velocity, and at a certain diſtance. For this purpoſe, it is eaſily made to do its office either with a ſpring, or a weight, fixed to one end of a wire, and a light ball at the other end of it, with a ſpring catch, to be made to let go its hold, when the diſcharge of a battery is wanted. Such an one is adapted to the preſent machine, and is fixed, by a cramp-iron and ſcrew, upon the table on which the electrical apparatus is placed, at the diſtance of about ſixteen or eighteen inches from the place where the diſcharge is to be made, ſo that no part of it be ſo near to any part of the apparatus that is electrified, as to ſteal away, or diſperſe any part of the charge. The ſpring-catch is let looſe by a ſtick of glaſs in the hand, ſo that there is no danger of being affected by the ſtroke.

Plate 3, fig. 1, ſhews this piece of apparatus, or fixed diſcharging rod. *A*, is the foot of it. *B*, is a ſtrong piece of glaſs, fixed in a wooden ſocket, which ſcrews into the foot *A*, to inſulate the upper part of it. *C*, is a cap of box-wood, with a ſocket in it to be put upon the top of the inſulating glaſs *B*, equal in height to the prime conductor of the machine. *D*, is a piece of ſtout wire, bent to a right angle, which goes through a hole in the cap *C*, and turns freely in it. The wire *E*, with its ball at one end, is ſcrewed into the end of the ſtout wire *D*; the wire *E*, together with its ball, is about ſixteen inches long. The wire *F*, has a ball of lead fixed to one end

Plate 3

Fig. 1

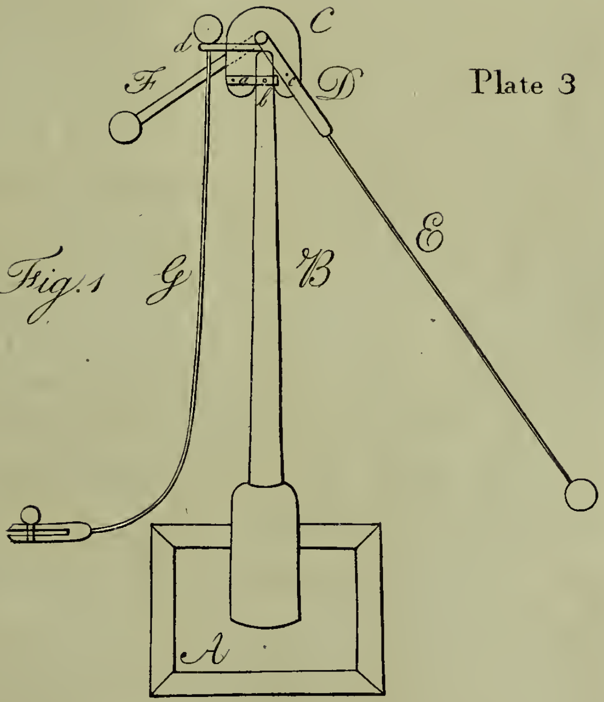


Fig. 2

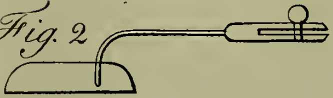


Fig. 3

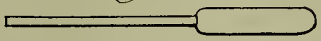
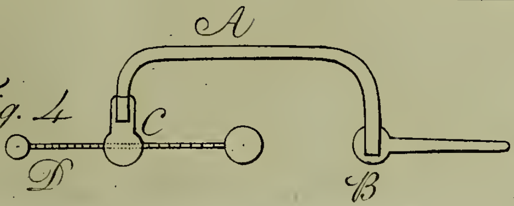


Fig. 4



end of it, the other end is fixed to the end of the ſtout wire *D*, at the back of the cap *C*, nearly at a right angle with the wire *E*. Towards the lower part of the cap *C*, is fixed to it, the ſpring-catch *a*, which has a hole in it at *b*; the wire *D*, has a pin fixed in it at *c*, which projects a little way out at the back-part of it, ſo as that when the wire *E*, is put perpendicular, the wire *F*, will be nearly horizontal, and the pin in *D*, will catch into the hole at *b*, in the ſpring catch *a*. When the ſpring *a*, is preſſed back, the pin *c*, will let go its hold, and the ball on the wire *F*, falling downwards, will throw the arm, or wire *E*, upwards, ſo as to paſs by the prime conductor, or any other part where the diſcharge is deſigned to be made, at a certain diſtance from it, and make the diſcharge of a battery, &c. At *d*, is a wire, one end of which goes into the cap *C*, far enough for the ſtout wire *D*, to reſt upon it as it turns, on making the diſcharge, in order to make the metalic communication complete through the cap *C*: the other end terminates in a ring: through this ring goes the wire *G*, with the ball on its upper end reſting upon the ring, and moves freely in it: on to the lower end of the wire *G*, is ſcrewed one of the ſcrew forceps, which receives one end of a wire, that is to be tried what force, or charge, it will bear before it will be melted, or deſtroyed.

Fig. 2, is a lump of lead, which is put any where upon the metalic covering on which the bottles of the battery ſtand: a hole is made in the lump of lead to receive one end of a wire, to the other end of which, is ſcrewed the other ſcrew forceps, which holds the other end of a wire, as the former.

Fig.

In Mr. Adams's firſt edition of his very uſeful *Eſſay on Electricity*, in the contents of the book, Chap. XVI. he ſays, *Mr. Brooke's Electrometer deſcribed,* but I cannot perceive how any perſon who had not ſeen my electrometer, could make out any thing of what mine is, either from what is given in that work plate 5, fig. 96, and 97, or by what is there ſaid concerning it. That which is repreſented by fig. 96, is of the Gentleman's invention, mentioned in the preceding notes, which, he was ſo obliging as to ſhew to me, when he had nearly finiſhed it, and which he afterward told me he had preſented to Mr. Adams: who might be under an injunction not to add the inventor's name to it. The arms on that repreſented at fig. 97, have a reſemblance to two of the arms

Fig. 3, is a piece of glaſs fixed into a wooden handle, to preſs back and diſengage the ſpring-catch before-mentioned.

arms on mine, but the method there given to graduate the uppermoſt arm, is very different from the method that I uſed to graduate mine; nor is that method, in my opinion, ſo accurate; for unleſs, in that method, the weights that are to be laid upon the ball be put exactly in a line over the points of ſuſpenſion, the graduations muſt be very irregular. In the method I uſed, no ſuch irregularity can ariſe, if any tolerable care be taken about it. In the third edition of Mr. Adams's Book, this article is wholly left out in the table of contents, and only noticed in the index to the work; where it is ſaid, under the article ELECTROMETER, *new one ſimilar to Mr. Brooke's:* but the ſimilarity, there ſpecified, ſeems to remain the ſame as it was in the former editions, both in the plates to the work, and in what is there ſaid about it.

CHAPTER II.

MISCELLANEOUS EXPERIMENTS

ON

ELECTRICITY.

EXPERIMENT I.

WITH a battery of nine bottles, containing about ſixteen ſquare feet of coated ſurface, charged to thirty-two grains of repulſion, which charge was ſent through a piece of *ſteel* wire twelve inches long, one hundredth of an inch thick, eleven times, when it was ſhortened one inch and a half, being then about ten inches and a half long; the twelfth time, the wire was melted to pieces.

EXPERIMENT II.

A charge, with the ſame nine bottles, to thirty-two grains of repulſion, being ſent through a piece of *ſteel* wire twelve inches long and one hundred and ſeventieth of an inch thick, the firſt time melted the whole of it into ſmall globules.

EXPERIMENT III.

A charge of the ſame nine bottles charged to thirty-two grains, being ſent through a piece of *braſs* wire twelve inches long, one hundred and ſeventieth of an inch thick, the whole of it was melted, with much ſmoke, almoſt like gunpowder, but the metallic part of it after it was melted, formed itſelf, in cooling, chiefly into concave hemiſpherical figures of various ſizes.

EXPERIMENT IV.

With only eight of the above bottles charged to thirty-two grains, the charge did but juſt melt twelve inches of the *ſteel* wire one hundred and ſeventieth of an inch thick, ſo as to fall into ſeveral pieces, which pieces in cool ing, formed themſelves into oblong lumps joining to each other by a very ſmall part of the wire between each lump, which was not melted enough to ſeparate, but appeared like oblong beads on a thread at different diſtances.

EXPERIMENT V.

The ſame eight bottles charged to thirty-two grains, ſo perfectly heated twelve inches of *braſs* wire, about one hundred and ſeventieth of an inch thick, as to melt it, or ſoften it enough for it to fall down by its own weight (from the forceps with which it was held at each end) upon a ſheet of paper placed under, to catch it; and when it fell down, it was ſo perfectly flexible, that by falling, it formed itſelf into a bent, or rather, vermicular ſhape, and remained entire its whole length, i. e. about twelve inches when it was put into the forceps; but after it was fallen on the paper, it ſagged ſo much as to be ſtretched by its own weight from twelve to about fifteen inches long; and by falling on the paper it flattened itſelf the whole length of it, that when it was examined with a half inch magnifier, it appeared about five or ſix times broader than it was in thickneſs.

EXPERIMENT VI.

With nine bottles again, charged only to twenty grains, the charge was ſent through twelve inches of *ſteel* wire, one hundred and ſeventieth of an inch thick, which heated it enough to melt it, ſo as to be ſeparated in many places; and the pieces formed themſelves into ſtrung-bead-like ſhapes as in Experiment 4.

EXPERIMENT VII.

With the ſame nine bottles charged to twenty grains, the charge was ſent through ten inches of *braſs* wire one hundred and ſeventieth of an inch thick, the wire was heated ſo red hot, as to be very flexible, yet, it did not ſeparate, but was ſhortened near three eighths of an inch.

REMARK I.

In the ſeventh experiment, the charge was not ſtrong enough to heat the ten inches of *braſs* wire, ſo as to be ſufficiently ſoftened, that it might ſtretch as in Exp. 5, but the flexibility was very plain, by its being made nearly ſtraight by the ſtroke.

EXPERIMENT VIII.

A charge of nine bottles, charged to twenty grains ſent a ſecond time through the laſt piece of wire, melted it aſunder in three places.

REMARK II.

The nine bottles charged to only twenty grains, in Exp. 6, 7, 8, ſeem to be of nearly the ſame power as eight of them charged to thirty grains, as in Exp. 4 and 5, but not exactly ſo, in Exp. 7, where the wire was *braſs*, as in Exp. 5, the quantity of coated ſurface was one ninth more than the fifth Experiment, though the charge was not ſo high by one third, which is a great diſproportion; but the length of the wire was leſs by one-ſixth, which brings it much nearer; as will be explained afterward.

EXPERIMENT IX.

Nine bottles charged to thirty grains and the charge ſent through twelve inches of *braſs* wire, one hundred and ſeventieth of an inch thick, treated it nearly as in Exp. 5, except that it was ſeparated in two places, and the pieces meaſured about ſixteen inches and a half long; but perfectly flattened by its fall on the paper, as before.

EXPERIMENT X.

Nine bottles charged to thirty grains, and the charge being ſent through eight inches and a half of *braſs* wire the ſize of the laſt, wholly diſperſed it in ſmoke, and left nothing remaining to fall on the ſheet of paper, placed under it.

REMARK III.

Experiments the ninth and tenth ſeem to ſhew, that ſomething very conſiderable is ſuperinduced or avoided, by the wire being longer, or ſhorter, with reſpect to its deſtruction, or preſervation, ſee Remark 2.

EXPERIMENT XI.

With twelve bottles, charged to twenty grains, the charge was ſent through ten inches of *ſteel* wire, one hundredth of an inch thick, which made the wire red hot, but did not melt it.

EXPERIMENT XII.

A ſecond charge, the ſame as the laſt, was ſent through the ſame piece of wire which heated it red hot as the firſt did, but it was not ſeparated; this piece of wire was now ſhortened five ſixteenths of an inch.

EXPERIMENT XIII.

A charge, to twenty-five grains, with the ſame twelve bottles, was ſent through the laſt piece of wire, which melted it into many pieces, and many globules of calcined metal.

EXPERIMENT XIV.

A charge of fifteen bottles, charged to twenty-five grains, was ſent through ten inches of *ſteel* wire one hundredth of an inch thick, which melted it the firſt time, and diſperſed a great part of it about the room.

EXPERIMENT XV.

A charge with the laſt fifteen bottles, charged to twenty grains, juſt melted ten inches of *ſteel* wire, the ſize of the former, ſo as to run into beautiful globules, nearly as in Exp. 13.

REMARK IV.

This laſt Experiment ſo nearly reſembles the 13th, that, excluſively of the wire having received two ſtrokes before they perfectly agree. In this laſt experiment, the quantity of coated ſurface was one fifth part more than in the former, but the height of the charge was one ſifth part leſs: in the former, the height of the charge was one fifth part more, but the quantity of coated ſurface was one fifth part leſs than the latter; ſo that the deficiency on one ſide, in either caſe, is made up by the exceſs on the other.

EXPERIMENT XVI.

A charge of fifteen bottles, charged to fifteen grains, being ſent through ten inches of *ſteel* wire, the ſize of the laſt, it was barely made red hot; but it was ſhortened one-tenth of an inch, by the ſtroke paſſing through it.

EXPERIMENT XVII.

The laſt piece of wire having a charge of fifteen bottles, charged to twelve and a half grains, ſent through it, did not make it red hot.

EXPERIMENT XVIII.

A charge of the ſame fifteen bottles, charged to twenty-five grains was ſent through the ſame piece of wire, which, ſeemingly, tore the wire into ſplinters.

REMARK V.

This wire did not run into globules when it was ſeparated, as in Exp. 15. The five grains, in this laſt, above the former, ſeem to have made the charge ſo ſtrong, as to act upon the wire inſtantaneouſly: whereas, in moſt of the experiments, where the wire was melted into globules, the heat does not arrive at its height ſo rapidly; but its progreſs is often ſo ſlow as to make it very pleaſing when the fuſion comes on.

EXPERIMENT XIX.

Four bottles, charged to thirty grains, juſt melted three inches of *ſteel* wire, one hundred and ſeventieth of an inch thick, ſo as to fall into pieces.

EXPERIMENT XX.

Five bottles, charged to twenty-five grains, moſt beautifully melted three inches of ſuch wire as the laſt, into large globules.

REMARK VI.

The four bottles did not do quite ſo much as the five, in the two laſt experiments, although they were charged higher: the charge of the former was higher by one ſixth part than the charge of the latter, but the quantity of coated ſurface in the five was one fifth more than in the four, ſo that the increaſe of ſurface in the five was ſomething more in proportion than the increaſe of charge in the four. It is very probable that if the four bottles had been charged a ſingle grain higher, their effect might have been equal to that of the five.

EXPERIMENT XXI.

Eight bottles, charged to fifteen grains, melted three inches of *ſteel* wire one hundred and ſeventieth of an inch thick, ſimilar to the five in the laſt experiment; ſo nearly alike both in appearance and effect, that it might have been ſaid to be the ſame.

EXPERIMENT XXII.

Ten bottles charged to twelve and a half grains, rather exceeded Exp. 19, but ſcarcely came up to Exp. 20, and 21.

EXPERIMENT XXIII.

Suſpecting ſomething in Exp. 19, I found, that though my bottles hitherto, were as nearly of the ſame ſize as I could procure them, yet, ſome of them were a little larger than others, and, which was the caſe in Exp. 19, one of the four was ſmaller than the other three, ſo that I repeated the experiment with four bottles more equal in ſize, and charged them to thirty grains, and the fuſion was as perfect as in any.

REMARK VII.

In Exp. 22, there were two ſmall bottles amongſt them: one of theſe ſmall ones, was rather larger than the other; had I taken care to adjuſt the increaſe and decreaſe of charge, exactly in proportion to the augmentation and diminution of ſurface, the laſt five experiments probably would have been, both in appearance and effect, ſo much alike as ſcarcely to have been diſtinguiſhable.

EXPERIMENT XXIV.

A charge to thirty grains with the ſame four bottles as in Exp. 19, was ſent through a piece of *ſteel* wire ſix inches long and one hundred and ſeventieth of an inch thick; but it would not melt the wire this length; it was but barely made red, or rather of a purpliſh red.

EXPERIMENT XXV.

A charge to twenty-five grains with the ſame five bottles as in Exp. 20, affected ſix inches of wire like the laſt, very nearly in the ſame manner, as in the preceding experiment.

EXPERIMENT XXVI.

A charge to fifteen grains with the ſame eight bottles as in Exp. 21, only heated ſix inches of wire like the laſt, juſt as in Exp. 24.

REMARK VIII.

The power of the charge in either of the three laſt experiments ſeems to be almoſt alike, and the only apparent cauſe why the wire was not melted as in Exp. 19, 20, 21, was the difference in the length of the wires, ſee Remark 2 and 3.

EXPERIMENT XXVII.

A charge to twenty grains, with the laſt eight bottles, very finely melted ſix inches of *ſteel* wire one hundred and ſeventieth of an inch thick.

EXPERIMENT XXVIII.

With two bottles, charged to forty-five grains, the charge was ſent through one inch of ſuch ſized *ſteel* wire as the laſt, which only changed its colour.

REMARK IX.

The changing of the colour in the laſt experiment ſeems to ſhew that the charge paſſed chiefly through the wire, as the ſpark, or light of the charge was not viſible at the wire, although the ends of the forceps were ſo near to each other.

EXPERIMENT XXIX.

Three bottles, with a forty grains charge diſperſed one inch and a half of *ſteel* wire, the ſize of the laſt, all about the room.

EXPERIMENT XXX.

As a *ſteel* wire of one hundredth of an inch thick has nearly double the quantity of metal of a wire one hundred and ſeventieth of an inch thick, ſo I took three inches of the former, and ſent a twenty-five grains charge with ten bottles through it, which melted it, juſt as the five bottles did in Exp. 20.

EXPERIMENT XXXI.

Twenty bottles charged to twelve grains and a half, melted three inches of *ſteel* wire, the ſize of the laſt, exactly ſimilar to the foregoing experiment.

EXPERIMENT XXXII.

As a *ſteel* wire of one eightieth of an inch thick contains nearly twice the quantity of metal in the ſame length as a *ſteel* wire of one hundreth, or four times the quantity of a *ſteel* wire of one hundred and ſeventieth of an inch thick, ſo it might, from the foregoing experiments, be expected that twenty bottles, charged to twenty-five grains, would melt three inches of *ſteel* wire one eightieth of an inch thick, but on a great many trials twenty bottles could not be procured that would bear the diſcharge, when charged to twenty-five grains; for at the diſcharge there would be always one, or more bottle or bottles broken, or perforated. I was now reduced to the neceſſity of being content with getting bottles of any ſize that would bear the required charge, from one to three gallons each, or that contained from about one hundred and fifty, to three hundred, or more, ſquare inches, of coated ſurface, each; but all in vain, my only reſource left (as I was not near any glaſs-houſe) was, to increaſe the quantity of ſurface, and not to charge ſo high, and to proportion the one to the other, [ſee Remark 6,] a third part was concluded on to be tried, that is, inſtead of about thirty-ſix feet of coating, I added one third, or, twelve feet, which made it forty-eight feet: and that, inſtead of charging to twenty-five grains (or twenty-four grains) which divides by three better, to omit one third of the height of the charge, which leaves ſixteen grains, and thus I ſucceeded perfectly well; for three inches of *ſteel* wire one eightieth of an inch thick was as curiouſly melted with forty-eight feet of coated ſurface, charged to ſixteen grains, as any of the former*.

REMARK X.

Theſe bottles, thus broken in large diſcharges, ſeem always to break, or to be ſtruck through, nearly in the thinneſt, but never in the thickeſt place, which ſhews the neceſſity of ſubſtance in the glaſs. [See introduction to the work.]

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* The deſtruction of ſo many bottles in the laſt experiment ſo much altered the battery, which hitherto was not ſo commodious as could be wiſhed, that I reſolved to make the arrangement anew, and in the order, mentioned in the introduction to the work, by which any ſingle bottle or any number of them may be diſengaged very readily without removing any of the other, and in this form it has been continued.

EXPERIMENT XXXIII.

As in Exp. 19, and 21, where the former is but half the quantity of coated ſurface of the latter, charged to thirty, and the latter to fifteen grains, to know how high forty-eight feet of coating muſt be charged to produce the ſame effect exactly: and as the quantity of coating in four bottles, conſiſting of a little more than ſix feet and a half, is contained in forty-eight feet a little more than ſeven times; ſo I tried by charging forty-eight feet only to a little more than four grains, or only about one ſeventh part ſo high, as four times ſeven is twenty-eight, that is, but two leſs than thirty, and this had exactly the ſame effect on the wire, which was one hundred and ſeventieth of an inch thick, and three inches long, as the former.

EXPERIMENT XXXIV.

As the laſt experiment agreed ſo exactly with Exp. 19 and 20, the next thing tried was, to ſee the effect of forty-eight feet of coated ſurface charged to a little more than four grains, upon ſix inches of *ſteel* wire, the ſize of the laſt; but this was only made very faintly red.

EXPERIMENT XXXV.

A repetition of the laſt experiment with the ſame length of the ſame wire, to ſee how often the ſame charge might be ſent through before it would be melted, and to obſerve the appearance of the wire after each ſtroke. The eighth ſtroke melted it into ſeveral pieces. After the firſt ſtroke the redneſs grew leſs every time, even the laſt time, when it was ſeparated. The firſt ſtroke though little more than fairly red, made it ſo flexible, that by a little more than its own weight, (about a penny-weight more) it was apparently made perfectly ſtraight when it was cooled: about the third or fourth ſtroke it began to appear zigzagged; after the ſixth ſtroke the ſurface of it appeared rough; after the ſeventh ſtroke the ſurface was very roughly ſcorified, or ſcaly; and ſome of the ſcales had fallen off upon a piece of white paper, placed under it, at about half an inch diſtance below it. The eighth ſtroke melted it in three places, and at thoſe places where the angles appeared the ſharpeſt, or moſt acute; a great number of the ſcaly appearances were driven off about the paper, which appeared like ſplinters, [ſee Exp. 18.] ſome of them were almoſt one tenth of an inch long, and ſome of them about a third or a fourth part of the diameter of the wire in breadth, and very thin: after the ſeventh ſtroke it was ſhortened ſeven ſixteenths of an inch: the wire was one hundred and ſeventieth of an inch thick.

EXPERIMENT XXXVI.

Repeating Exp. 34, again, with the ſame ſize, and length of wire, and the ſame battery charged the ſame, in order to obſerve the method of the wire ſhortening, having fixed an inſulated gage parallel to, and about a quarter of an inch diſtant from it: after the firſt ſtroke, which made the wire fairly red, (it being fixed at one end, that the ſhortening might appear all at the other, which was held ſo as either to contract or dilate) I obſerved that it ſhortened conſiderably as it cooled; repeating the ſtroke, it did the ſame, and ſo on till it was melted, which was by the eighth ſtroke, as before. At the inſtant that the ſtroke paſſed through the wire it appeared to dilate a little, and after it was at its hoteſt, it gradually contracted after every ſtroke as it cooled, about one ſixteenth of an inch each time, the dilating was ſo very little, as to bear but a very ſmall proportion to its contraction, and ſometimes it was doubtful whether, or not, it did dilate at all; but after all the obſervations it appeared oftener as if it did dilate, than as if it did not.

EXPERIMENT XXXVII.

The ſame battery with forty-eight feet of coated ſurface charged to ſix grains, melted ſix inches of *ſteel* wire one hundred and ſeventieth of an inch thick, the moſt pleaſingly into the fineſt ſtrung-bead ſhapes that I have hitherto obſerved.

EXPERIMENT XXXVIII.

The ſame forty-eight feet battery, charged to ſeven and a half grains, very finely melted twelve inches of *ſteel* wire the ſize of the laſt.

EXPERIMENT XXXIX.

The ſame forty-eight feet again, charged to ſix grains, would not make twelve inches of wire like the laſt, red hot.

EXPERIMENT XL.

The ſame forty-eight feet *negatively* charged to a little more than four grains, melted three inches of *ſteel* wire, one hundred and ſeventieth of an inch thick the ſame as the *poſitive* charge did in Exp. 33.

EXPERIMENT XLI.

The ſame battery of forty-eight feet of coated ſurface, charged to a little more than eight grains, melted three inches of *ſteel* wire, one hundredth of an inch thick. This is very nearly in proportion to Exp. 30, but here the charge was *negative*, and the fuſion was the moſt pleaſing of any I have hitherto had: probably owing to the charge, by chance, happening to be ſo well adjuſted as to be exactly ſufficient to melt the wire and no more: it held hot the longeſt, and the fuſed metal ran into the largeſt globules: probably the length of the time that the heat continued, was owing to the charge being juſt ſufficient, and to the ſize of the lumps that the fuſed metal formed itſelf into.

REMARK XI.

The ten bottles in Exp. 30, were of ſuch a ſize that the contents of their coated ſurface was very near one third part of forty-eight feet, equal to the contents of coated ſurface in the battery uſed in the laſt experiment, that is, about eighteen feet, but the charge of the former being two thirds higher, made their effect ſo very nearly equal.

EXPERIMENT XLII.

Forty-eight feet as in laſt experiment, charged to nine grains, melted eighteen inches of *ſteel* wire one hundred and ſeventieth of an inch thick; it was melted at both ends into globules, and ſtrung-bead ſhapes about four, or five inches at each end, but there remained about nine inches unſeparated in the middle part of the wire, the ends of it were clearly made much hotter than the middle, which was made only fairly red, and cooled very ſoon; nevertheleſs, it was ſo heated, as to calcine, or ſcorify, the outſide of the wire the whole length that remained unſeparated; and when it was taken up, by handling, it bent angularly, and at every bending, the calcined, or ſcorified, metal broke away and fell off, leaving the internal part of the wire quite perfect, except that it was much diminiſhed by the ſcorification; but whilſt in the hands, it appeared, between the bendings, to be enlarged, about one third part in thickneſs.

EXPERIMENT XLIII.

A repetition of Exp. 1, with twelve inches of *ſteel* wire, one hundredth of an inch thick, but with this difference, that, as then I uſed only nine bottles, containing about ſixteen ſquare feet of coated ſurface charged to thirty-two grains, I here uſed eighteen bottles containing about thirty-two ſquare feet of coating charge dto only ſixteen grains. This was done, to obſerve the progreſs of the deſtruction of the wire, as in Exp. 35, as well as to prove the ſimilarity of the effect. The wire being the ſame ſize, ſort of metal, and length, as recited juſt above; the firſt ſtroke made it fairly red hot the whole length of it with ſmoke and ſmell, changed its colour to a kind of copperiſh hue, and ſhortened it conſiderably; the ſecond ſtroke made it of a fine blue, but it did not appear red, and ſhortened it more; at the third ſtroke, it became zig-zagged, many radii were very viſible at the bendings, and continued to ſhorten till the eleventh ſtroke, when one of the bottles in the ſecond row of the battery was ſtruck through, the fracture was covered over with common cement*, its place ſupplied by changing place with one in the third row, ſuppoſing the mended one to be the weakeſt, [ſee the introduction to the work] and thus, with the battery in this ſtate, I made the twelfth ſtroke, which ſeparated the wire, as in Exp. 1, but this wire was ſhortened only one inch.

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* This cement is very cheap and eaſily made, as follows: take eight ounces of ſpaniſh white, heat it very hot in an iron ladle, to evaporate all moiſture, and when cool, ſift it through a lawn ſieve, then add three ounces of pitch, three quarters of an ounce of roſin, half an ounce of bees-wax, heat them altogether over a gentle fire, till the mixture is pretty hot, frequently ſtirring it, for near an hour; then take it off the fire, continue the ſtirring till it grows cool, and it is fit for uſe.

REMARK XII.

The ceaſing of the appearance of the wire being heated red hot after the firſt ſtroke, ſeems to be owing to the firſt ſtroke, in its paſſage, calcining the outſide of the wire to a certain depth or thickneſs, which calcination remaining upon the wire, conceals the redneſs at the ſecond and ſucceeding ſtrokes. In Exp. 42, is a ſufficient proof of the calcining the outer ſurface of the wire when it only appears fairly red hot: in that experiment is a proof of various degrees of heat, produced at the ſame time, from perfect deſtruction of the wire to its appearing only fairly red hot. What advantage ſuch experiments as this may be of, in determining the direction of the electricity, time and perſeverance may ſhew.

EXPERIMENT XLIV.

A repetition of Exp. 43, with the ſame eighteen bottles, charged the ſame, and a piece of wire the ſame length and ſize; relative to the wire being heated red hot the firſt time and not after, till the twelfth ſtroke, when it was melted. The firſt ſtroke made it red hot, and produced much ſmoke, as before, but no ſmoke or redneſs at the ſecond ſtroke; after the firſt ſtroke it was of a copper colour, but of a fine blue after the ſecond, and began to be zigzagged, it was likewiſe ſhortened one tenth of an inch, by theſe two ſtrokes.

EXPERIMENT XLV.

With the laſt eighteen bottles charged to twelve grains, the charge was ſent four times through ten inches of *braſs* wire, one hundred and ſixtieth of an inch thick; the fifth ſtroke ſeparated it, and ſhortened it thirteen ſixteenths of an inch, it was alſo made red hot by every ſtroke.

EXPERIMENT XLVI.

A twelve and a half grains charge with eighteen bottles, ſent through three inches of *ſteel* wire, one hundred and twentieth of an inch thick very finely melted it.

EXPERIMENT XLVII.

A twelve and a half grains charge with forty-eight feet of coated ſurface was made to paſs through three inches of *braſs* wire, one hundred and twentieth of an inch thick, which juſt heated it red hot enough to fall into ſeveral pieces.

REMARK XIII.

From the two laſt experiments it appears, that a *braſs* wire, of the ſame length and ſize of a *ſteel* one, will bear a charge one third part ſtronger.

EXPERIMENT XLVIII.

A charge of eighteen bottles, or about thirty-two ſquare feet of coated ſurface, charged to ten grains, ſent through three inches of *ſteel* wire, one hundred and twentieth of an inch thick, heated it fairly red hot.

EXPERIMENT XLIX.

The ſame eighteen bottles charged to eleven grains, the charge being ſent through the former piece of wire, melted it into ſeveral pieces; but thoſe parts that remained in the form of wire were very much ſhivered as in Exp. 18, and 35.

EXPERIMENT L.

The laſt eighteen bottles charged to eleven grains, and the charge ſent through three inches of *ſteel* wire the ſize of the laſt, heated it very red hot, but did not ſeparate it.

EXPERIMENT LI.

A charge of forty-eight feet, to eleven grains, heated three inches of *braſs* wire, one hundred and twentieth of an inch thick red hot, but would not ſeparate it, for it remained whole in the forceps after the ſtroke had paſſed through; but it was ſo very near being ſeparated by the ſtroke, that it parted on letting it out of the forceps: the ſtroke ſhortened the wire one twentieth of an inch.

REMARK XIV.

The two laſt experiments prove what is advanced in Remark 12, but the *braſs* wire does not appear to ſcorify with the ſtroke ſo much as *iron* or *ſteel.*

EXPERIMENT LII.

A charge of forty-eight feet to eight grains ſent through three inches of *copper* wire, one hundred and ſeventieth of an inch thick, ſeven times, made it zigzagged, but not much ſhorter, the eighth ſtroke ſeparated it at one end, cloſe to the forceps which held it, but it did not appear to be made ſenſibly red hot at all, notwithſtanding it muſt have been often ſo at the place where it was melted: which ſpace was ſo very ſmall as barely to be perceptible, like as when a point is ſet upon any flat ſurface of *iron,* and a ſtroke from a pound phial being ſent through, both the point and the flat ſurface where the point reſted, if examined with a magnifying glaſs, will be found to have been melted, and a ſpeck may be ſeen;

but the redneſs of the metal will ſcarcely be viſible.

REMARK XV.

As obſerved in the laſt experiment with a point and a pound phial, I have often melted holes in a thin ſheet of *braſs*, ſuch as thoſe from which grain weights are cut.

EXPERIMENT LIII.

A repetitition of the laſt experiment, only adding two grains to the charge, that is, ten grains, inſtead of eight, as in the laſt, with a piece of *copper* wire the ſame length and ſize, which was, by the charge, diſperſed all about the room.

EXPERIMENT LIV.

A charge of forty-eight feet to twelve grains, ſent through three inches of *copper* wire, one hundred and fortieth of an inch thick, heated it red hot, enough to ſeparate it in ſeveral places.

EXPERIMENT

EXPERIMENT LV.

A repetition of the laſt experiment, with the ſame ſucceſs, every circumſtance being alike both in the wire and the charge.

EXPERIMENT LVI.

A charge of forty-eight feet to ſix grains ſent through three inches of *ſteel* wire, one hundred and ſeventieth of an inch thick, diſperſed the whole of it in red hot globules about the room.

REMARK XVI.

The additional reſiſtance that is made, to the electrical ſtroke, by the additional length of a wire, as obſerved in Remark 3, very plainly appears in this laſt experiment; and the obſervation ſeems to hold good either with large or ſmall quantities of coated ſurface.

EXPERIMENT LVII.

A charge of forty eight-feet to five grains, ſent through three inches of *ſteel* wire, one hundred and fortieth of an inch thick, heated it red hot ſo that the ſurface of the wire was ſcorified its whole length; it did not melt it into pieces, but ſhortened it one twentieth of an inch.

EXPERIMENT LVIII.

A charge of forty-eight feet to ſix grains, and ſent through three inches of *ſteel* wire one hundred and fortieth of an inch thick, melted it into fine globules.

REMARK XVII.

Hence it appears that a piece of *copper* wire, the ſame ſize and length, will bear twice the charge that a *ſteel* wire will, [ſee Experiment 54.]

EXPERIMENT LIX.

A repetition of the laſt experiment having every circumſtance alike, and the effect was the ſame: this repetition was only to aſcertain the fact more fully.

EXPERIMENT LX.

A charge of forty-eight feet of coated ſurface to between ten and eleven grains was ſent through three inches of *ſteel* wire, one hundredth of an inch thick, which juſt melted it ſo as to ſeparate.

REMARK XVIII.

Hence it ſeems as if a *copper* wire of one hundred and fortieth of an inch thick, would bear as great a charge as a *ſteel* wire of double the ſize and equal in length; or that the *copper* wire would be rather the ſafeſt.

EXPERIMENT LXI.

A charge of forty-eight feet, charged to twelve grains, being ſent through one inch and a half of *ſteel* wire, one hundredth of an inch thick, the wire was diſperſed all about the room.

EXPERIMENT LXII.

The ſame charge as the laſt was ſent through one inch and three eighths of *copper* wire one hundred and fortieth of an inch thick, but the charge did not ſeparate it.

REMARK XIX.

Now it ſeems to be confirmed, that a *copper* wire of almoſt any length and ſize, will be equally ſafe as a *ſteel* wire of the ſame length and double the ſize. And this wire not being melted as in Exp. 54, the charge and ſurface being equal, ſeems to be owing to the forceps being too near, for the whole of the charge to paſs ſolely by the wire, as in Exp. 28.

Hitherto it may appear as if more attention had been given to the differentſizes of wires (and the ſorts of metal) than to the length of them; but the knowledge of the latter, is as ſatisfactorily obtained, as the former, from the ſame experiments; for it appears from Experiments the 2d, 19th, and 21ſt, that a certain quantity of coated ſurface charged to a certain heighth, will melt a wire of a certain ſize and length of a certain metal. It appears likewiſe, from the ſame experiments, that double the quantity of coated ſurface, charged to the ſame height, will deſtroy four times the length of the ſame wire; as the length of the former, was four times that of the latter, and the quantity of coated ſurface in the former, was about double that of the latter; but the charge was nearly equal in both. Or the obſervation may be changed, thus, that, as already obſerved, a certain quantity of ſurface, charged to a certain height, will melt a wire of a certain ſize and length of a certain metal; ſo it appears from various other experiments, that the ſame quantity of ſurface, charged twice as high, will do the ſame execution, as twice the quantity of ſurface charged half as high.

REMARK XX.

At Experiments 33 and 34, let the mean of the length of the wires, in the two experiments, be taken; and it will agree, as near as poſſible, with Exp. 42, in the ſame proportion as the foregoing. In Exp. 33, the wire was three inches long, but it was all melted. In Exp. 34, the wire was ſix inches long, but none of it was melted. Now the mean length of the wires in the above two experiments is four inches and a half; which is juſt equal to one fourth part of the length of the wire in Exp. 42: and combining thoſe three experiments, the effect is produced as in experiments the 2d and 21ſt. And whenever theſe experiments ſhall be repeated, with the like apparatus, I am inclined to think that the reſult will be found very nearly the ſame.

EXPERIMENT LXIII.

A charge of forty-eight feet, to five grains, ſent through three inches of *lead* wire, one ſixty-ſixth of an inch thick, melted it into many different pieces, and lumps of lead.

EXPERIMENT LXIV.

A charge of forty-eight feet to four grains, melted three inches of *lead* wire, one ſixty-ſixth of and inch thick, aſunder, in three different places.

EXPERIMENT LXV.

A charge like the laſt was ſent through ſix inches of *lead* wire like the laſt, which only melted it aſunder about the middle of it.

EXPERIMENT LXVI.

A charge of forty-eight feet to eight grains was ſent through three inches of *lead* wire one fifty-ſixth of an inch thick, which melted it into many pieces.

EXPERIMENT LXVII.

A charge like the laſt was ſent through ſix inches of *lead* wire the ſize of the laſt, which only melted it aſunder in one place near the middle.

REMARK XXI.

As a *ſteel* wire of one fifty-ſixth of an inch thick appears to contain nearly four times as much metal in the ſame length as a wire of one hundredth of an inch thick; and as it took a charge of about eight grains with forty-eight feet of coated ſurface, to deſtroy three inches of the latter; [Exp. 41] ſo it appears that a *ſteel* wire would be four times as effectual againſt the electricity or lightning, as a *lead* wire of the ſame length and ſize.

EXPERIMENT LXVIII.

A charge of ten grains, from forty-eight feet, ſent through three inches of *lead* wire, one fiftieth of an inch thick, made no ſenſible alteration in it.

EXPERIMENT LXIX.

A charge of forty-eight feet, to twelve grains, ſent through three inches of ſuch *lead* wire as the laſt, melted it into ſeveral pieces.

EXPERIMENT LXX.

A charge like the laſt was ſent through ſix inches of ſuch wire as the laſt, which only melted it aſunder in one place.

EXPERIMENT LXXI.

A charge of forty-eight feet, to ſixteen grains, was ſent through ſix inches of *lead* wire, one fiftieth of an inch thick, which melted it into many pieces.

EXPERIMENT LXXII.

A charge of forty-eight feet, to fifteen grains, was ſent through ſix inches of wire like the laſt, which did not ſeparate it, but made it ſmoke.

EXPERIMENT LXXIII.

A charge like the laſt was ſent through the laſt piece of wire a ſecond time; which melted it into ſeveral pieces.

REMARK XXII.

The law by which wires reſiſt deſtruction, in proportion to the thickneſs of the wire, does not ſeem to be ſo equable, by much, in the *lead*, as in the *ſteel* wire. For a charge of four grains, in Exp. 64, melted three inches of *lead* wire, one ſixty-ſixth of an inch thick: but it took a charge of about three times that power to deſtroy three inches of *lead* wire one fiftieth of an inch thick; which is about double the quantity of metal in the ſame length as in that of one ſixty-ſixth of an inch thick. [ſee Exp. 69.] Thus it is eaſy to find, what different reſiſtance a wire of any of the foregoing metals, of equal ſize and length, will make to the electrical ſtroke, or to lightning.

The length of the electric circuit, in which the different wires were placed, in the foregoing experiments, from the neareſt part of the inſide, to the neareſt part of the outſide, of the battery, excluſive of the length of the ſaid wires, was about eight feet.

Notwithſtanding the eaſy deſtruction of the *lead* wire by the electrical ſtroke; it ſeems greatly to be doubted, whether any thunder ſtrokes happen in any place whatever, ſtrong enough to deſtroy a ſtrip of lead four inches broad and of the thickneſs of about eight pounds to the foot. Whence it may be preſumed, that ſuch a ſtrip of lead may be perfectly ſafe for conductors through buildings of any kind whatever: as it is not much ſubject to decay in any common expoſure.

The moſt remarkable inſtance of the effect of a thunder ſtroke, which ever happened in the limits of my knowledge, was at a ſtable belonging to the Rev. Dr. Sanby, at Denton, about fourteen miles from Norwich; which happened on the 28th of July 1775, at about ſix o'clock in the afternoon: where the gable end of the ſtable, which faced nearly weſt, was very much damaged; and on the top of the gable, which was brick work, was fixed a vane on an iron rod, whoſe length was about eight feet; and, for ſteadineſs, it was wrought up in the brick-work near three feet down. In the roof of the ſtable is a hay-loft; and about two feet below the floor of the hay-loft, about the middle, within the ſtable, into the brick-work, was driven a large iron ſpike; and upon this ſpike was hung a pair of iron *yangles*[*], as they are called. The ſtroke of the lightning ſeems to have taken its paſſage by this pair of *yangles* as they hung, and the rod of the vane; and in its paſſage the *yangles* were ſo compleatly fixed, at the joinings of the links, by being melted with the ſtroke, that the whole of the chain remained perfectly ſtiff and ſtraight, and adhered ſo firmly together, that the *yangles* were carried to the blackſmith's to be ſet looſe again, and made fit for uſe. The rod of the vane, and the vane itſelf, were wholly miſplaced, and never yet were found any where, nor is it expected now, will ever be found. Great part of the bricks of the gable were thrown in various directions; ſome of them were thrown over part of the dwelling-houſe, which was conſiderably higher than the ſtable, to the diſtance of between eighty and ninety yards; [which I meaſured by pacing the diſtance:] many of the windows of the dwelling-houſe, which was at about twent-four yards diſtance, were very much broken. A man ſervant, who was in the ſtable, was conſiderably benumbed, but recovered himſelf in a few hours after. There were, likewiſe, ſome horſes in the ſtable, but they received no hurt.

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[*] A kind of fetters made of ſtrong iron links, to fix on a horſes foot, to prevent them from running away or breaking paſture.

Hitherto, the ſtate of the clouds and the earth, in the time of thunder ſtorms, appears to be very far from being well aſcertained. But whoever has an inſulated conductor continued into a dry and cloſe room, may find, that changes in the air, or the rain, ſnow, or hail falling from the clouds, from negative to poſitive, and from poſitive to negative, happen ſo very often, that it does not appear eaſy to diſcover the ſtate of the earth at ſuch a time. Many of theſe changes may be obſerved to take place, by a pair of ſmall cork balls, ſuſpended on linen threads from the inſulated conductor, in the ſpace of ten minutes; particularly when the clouds and the thunder are near. When there is no thunder, or lightning, but only rain, ſnow, or hail, thoſe changes will often be not more than twice, thrice or four times during the whole time that the cloud is paſſing over, and ſometimes only once.

A little after five o'clock in the morning of July 7, 1784, there happened a very violent ſtorm of thunder and lightning at Bramerton, about four miles from Norwich, which did conſiderable damage at the dwelling-houſe of Mr. John Gurney; where the lightning ſeemed to take its courſe by a bell wire, part of which was braſs, and the other part iron. That of iron was of the ſize called No. 21, and the braſs No. 20, ſo that the braſs wire was the thickeſt; and it was melted into many pieces; theſe I picked up in different rooms which it went through; but the bell-wire being continued into a third room, and the continuation being of iron wire, and ſmaller than the braſs one, the whole of it was diſperſed in ſmoke; a great quantity of which adhered to the wainſcot of the room, and nothing elſe of the wire remained, except at the ends where it was faſtened to the bell-drags; but there, the wire was double, being twiſted one to the other, and both the iron and braſs remained unhurt. Theſe I preſerved in order to examine as to their ſize and weight. The braſs wire, which was juſt ſeparated, being the ſize of No. 20, I procured a piece of iron wire, the ſame ſize, and cut it three inches and a quarter long, which then weighed ſix grains and ſeventy-nine hundred and twenty-eighths of a grain, nearly ſix grains and five eighths.

By calculating from the foregoing obſervations, it appears that, for the iron wire to have been equal in power againſt the lightning to the braſs one, the ſize of it muſt have been ſo much larger that three inches and a quarter in length muſt weigh nearly nine grains; which appears to be a little thicker than that which is called No. 19.

It has already been obſerved that, double the quantity of ſurface charged the ſame, will deſtroy four times the length of wire. [ſee Remark 16.]

In Experiment 32, it plainly appears, that forty-eight feet of ſurface, charged to ſixteen grains, melted three inches of *ſteel*, or *iron* wire one eightieth of an inch thick; from which, if the rule holds good, eight hundred and ſixty-four ſquare feet of coated ſurface, charged to ſixteen grains, ſhould melt three inches of *iron* wire, equal in power againſt the lightning to the braſs one melted at Mr. Gurney's. And hence, preſuming the rule to be good in all caſes, which I hope will be tried, if Mr. Gurney's wires had all been iron, of the ſize of about No. 19, or equal in power to the braſs one, in preſerving itſelf, a charge of one thouſand four hundred and forty feet of coated ſurface* charged to ſixteen grains, having been made to paſs through their whole length, which altogether was about forty feet, would have deſtroyed them in the ſame manner as the lightning.

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* A ſurface of about thirty-eight feet ſquare.

The following is an inſtance which I never happened of before, or ever repeated.

Two Gentlemen coming in, to ſee a piece of wire melted by electricity, I proceeded to ſhew it them, by fixing twelve inches of *ſteel* wire, one hundred and ſeventieth of an inch thick, in the forceps, and then (ſuppoſing the electrometer and all other things ready placed) to charge the battery, but the electrometer did not move; nevertheleſs I continued charging, as I ſuppoſed; but ſtill the electrometer remained as it was, although I had been charging much longer than would have been neceſſary; contrary to my deſign, which was to take a ſmall wire, that a ſmall charge might be ſufficient. Having been charging a long time, I left off to look about the apparatus, in order to ſee if any thing was not right: as I was looking, I found there was no communication to the electrometer, and heard a ſmall crackling in the battery, which convinced me that it was charged. Accordingly I made the diſcharge, expecting nothing unuſual; but the wire was diſperſed ſeemingly in a very violent manner. The report was ſo very loud that our ears were ſtunned, and the flaſh of light ſo very great, that my ſight was quite confuſed for a few ſeconds. The ſingularity of the appearances attending this experiment led me to inſert it.

CHAPTER III.

MISCELLANEOUS OBSERVATIONS
ON THE
LEYDEN PHIAL.

NOTWITHSTANDING the number of inſtructive as well as entertaining experiments on electricity, and in particular thoſe, in which the Leyden Phial is uſed, moſt of thoſe Gentlemen who have been pleaſed to conſider me as one of their philoſophical correſpondents on this ſubject, ſeem always to have conſidered the outſide and inſide of the Leyden Phial, at the time that it is charging, to be in oppoſite ſtates, that is, one *negative*, and the other *poſitive*; and all of them quote Doctor Franklin, in ſupport of their opinion.

The late Mr. James Ferguſon* was firm in this opinion, till I ſhewed him that this was not the caſe.

Another Gentleman to whom I had mentioned it in the year 1775, writes as follows, '*I am aſtoniſhed at what you tell me, relating to*
'*a charged Leyden Phial, and long much to ſee*
'*your experiment. Till I do, I confeſs I cannot*
'*help thinking there muſt be ſome fallacy in it,*
'*which has eſcaped both yourſelf and Mr. Fergu-*
'*ſon. If however the fact be true, that both ſides*
'*of the phial are negative, or both ſides poſitive,*
'*the whole of the preſent theory muſt fall to the*
'*ground* †.'

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* Mr. Ferguſon gave a courſe of Lectures in Norwich in the year 1769, and the ſame again in April 1775.

† I had mentioned to this Gentleman that Mr. Ferguſon had been with me; but inſtead of my experiment militating againſt the preſent, that is, Dr. Franklin's Theory, I think it very ſtrongly confirms it.

Likewiſe that very excellent philoſopher and electrician, the late Mr. Wm. Bewly, ſeems to have been firmly eſtabliſhed in the ſame opinion, who, in the year 1780, in a letter to the Rev. Mr. Morgan, ſpeaking of two Leyden Phials, one hanging to the tail of the other ſuſpended from the prime conductor, and deſcribing them by calling the wire that enters the mouth of the uppermoſt, or firſt phial *P*, the wire that leads from the coating of the firſt, and enters the mouth of the ſecond phial *p*, the coating of the firſt *N*, and the coating of the ſecond *n*, with a chain leading from the laſt to the floor, writes as follows: ‘ *I wonder how Mr. Brook, with* ‘ *his well known experience in electricity, can conſider* ‘ *N and p, as in the ſame ſtate; to ſhew that* ‘ *they are not; it is ſufficient only to remove them* ‘ *from each other: when he will find that p is* ‘ *poſitive, and N negative he may take a ſhock* ‘ *from them by bringing them together. The puzzle* ‘ *ariſes from the ſame conducting ſubſtance being* ‘ *connected with both.*’

Is there not as much reaſon for any one to *wonder*, how *any electrician* can conſider any two perfect metallic ſubſtances in actual contact with each other, to be in different ſtates of electricity, except, as in Wilſon and Hoadly's Experiments; and I never ſaid that they were not ſo, when they were ſeparated. Mr. Bewly adds, '*it may be ſaid* '*there ought to be no exploſion between P and p*, '*becauſe they are both poſitive*,' this is certainly true, '*but p, has a metallic communication with* '*N, the negative outſide of the uppermoſt phial*,' here is a miſtake; for this outſide is not negative, at leaſt the coating of it, but poſitive, having a compleat communication with *p*, the poſitive inſide coating of the lowermoſt phial. '*The ſame reaſoning will apply to the exploſion* '*which takes place between N and n, n is un*-'*doubtedly negative*:' this is alſo certainly true: '*and ſo is N*,' but this is plainly a miſtake again, for *N* is not negative, as already obſerved.

In the preceeding ſection Mr. Bewly ſays, ‘*It may be ſaid there ought to be no exploſion between P and p, becauſe they are both poſitive,*’ but their being both in a poſitive ſtate does by no means ſhew that there ought to be no exploſion, when bottles are thus ſituated; for it is generally allowed, that where there are unequal portions of the electric fluid, either negatively, or poſitively, if a proper communication be made between them, an exploſion will take place: and this appears to be the caſe between *P* and *p*, at leaſt as to their coating; for admitting that no more of the electric fluid can be driven into the inſide of a bottle, than can be permitted to leave the outſide, and that that which is driven from the outſide of the firſt phial is driven into the inſide of the ſecond; and admitting that all the time the bottles are charging, each bottle will poſſeſs its natural quality; yet on ceaſing to charge them higher, or on ceaſing to work the machine, the portion which was driven from the outſide of the firſt phial into the inſide of the ſecond, will, by the metallic communication between them both, take place not only in the inſide of the ſecond phial, but will return and take place on the outſide of the firſt, at leaſt ſo far as their coatings are conſidered to be concerned, yet the inſide of the firſt continues as it was when the machine ceaſed working. Thus the inſide of the firſt phial will contain a quantity of the electric fluid nearly equal to the quantity occupying its own outſide and the inſide of the ſecond; therefore, by the inſide of the firſt being now ſo highly charged in proportion to its outſide, if a communication be made with an inſulated diſcharging rod from its wire to its coating, or from its coating to its wire, an exploſion will ſucceed, yet neither of the bottles will be diſcharged; but inſtead thereof, the inſide of the ſecond phial and both ſides of the firſt, whilſt the communication is continued, will all be charged equally high and of the ſame quality, that is, poſitive: ſuppoſing the rubber of the machine not inſulated. Thus it ſeems as if a coated glaſs phial may be made to contain more than its natural quantity; for it does not clearly appear, that the coating alone will contain all this additional quantity about the bottle.

Notwithſtanding all that has been done with, or ſaid about, the firſt phial, with reſpect to the great quantity that the inſide of it contained when the machine ceaſed working; a few experiments will ſhew, that the ſame inſide may be altered from being ſo ſtrongly poſitive, to negative, or in the ſame ſtate as the outſide of the ſecond phial at the time that it had a communication by a wire to the floor, without diſcharging the phial or any more working the machine, and will alſo, at the ſame time, afford many proofs of the truth of this ſeemingly new conceived opinion.

EXPERIMENT I.

For the ſake of convenience let the apparatus be a little altered; and inſtead of two phials being ſuſpended from the prime conductor, let two pound phials be coated with tin-foil on their outſides, and filled to a convenient height with common ſhot, to ſerve as a coating within-ſide, as well as to keep a wire ſteady in the phials without a ſtopple in the mouth of them. Let each phial be furniſhed with a wire, about the ſize of a gooſe quill, and about ten inches long, and let each wire be ſharpened a little at one end, that it may the more eaſily be thruſt down into the ſhot, ſo as not to touch the glaſs any where at the mouth of the phials, yet, ſo as to ſtand ſteadily in them. Let a metallic ball about 6 or 7 eighths of an inch diameter be ſcrewed on at the other end of each wire: alſo let there be in readineſs a third wire, fitted up like thoſe for the phials, except that another ball of nearly the ſame ſize as the former, may occaſionally be ſcrewed on over the ſharpened end of it. I ſay, inſtead of ſuſpending the phials from the prime conductor, as before, let one of thoſe above-deſcribed, be charged at the prime conductor, and then ſet it aſide, but let it be in readineſs in its charged ſtate: then let the other be placed upon a good inſulating ſtand, and let the third wire alſo be laid upon the ſtand, ſo that its ball, or ſome part of the wire, may touch the coating of the phial. Let the ſharpened end of this wire project five or ſix inches over the edge of the ſtand: all of theſe being now placed cloſe to the edge of a table, hang a pair of cork balls on the ſharpened end of the wire, and make a communication from the prime conductor to the ball on the wire in the bottle: on working the machine, the ſharpened end of the wire will permit the bottle to be charged although it be inſulated, and if the wire be very finely pointed, the bottle may be charged nearly as well as if it were not inſulated: I ſay, on working the machine, the phial will charge, and the cork balls will immediately repel each other: but whilſt this phial is charging take the firſt phial, which having been previouſly charged at the ſame prime conductor, in the hand, and while the ſecond phial is charging, preſent the ball of the firſt to the cork balls, and they will all repel each other. This plainly proves that the outſide of the ſecond bottle is electrified plus, at the time that it is charging, the ſame as the inſide of the firſt, and the inſide of both the bottles will readily be allowed to be charged alike, that is, plus, or poſitive.

EXPERIMENT II.

Let the ſecond bottle in the laſt experiment be wholly diſcharged, and charge it again as before, (the firſt bottle yet remaining charged) and whilſt it is charging, let the ball of the firſt approach the cork balls contiguous with the ſecond, and they will, as before, all repel each other: withdraw the ball of the firſt, and ſo long as the machine continues to charge the ſecond bottle higher, the cork balls will continue to repel each other, but ceaſe working the machine, and the cork balls will ceaſe to repel each other till they touch, and will then very ſoon repel each other again; then let the ball in the firſt phial approach the cork balls, and they will now be attracted by it, inſtead of being repelled as above, as in the laſt experiment. This alſo plainly ſhews, that both ſides of a Leyden

Phial are alike at the time it is charging, and at the ſame time evidently ſhews, that the difference of the two ſides does not take place till after the bottle is charged, or till the machine ceaſes to charge it higher.

EXPERIMENT III.

In this experiment, let both the former bottles be diſcharged, then let one of them be placed upon the inſulating ſtand. Let a ball be put on over the ſharpened end of the third wire, and let it be laid on the ſtand as before, ſo as to touch the coating of the phial: place the other phial on the table, ſo that its ball or wire may touch the ball on, or any part of, the third wire: make a communication from the ball on the wire of the firſt phial, to the prime conductor: then, by working the machine, both bottles will ſoon become charged. As ſoon as they are pretty well charged, and before the machine ceaſe working, remove the ſecond phial from the third wire; after the ſecond phial is removed, ceaſe working the machine as ſoon as poſſible:

take the third wire, with its two balls, off the ſtand with the hand, and lay it on the table, ſo that one of its balls may touch the outſide coating of the ſecond phial: remove the firſt phial off the ſtand, and place it on the table ſo as to touch the ball at the other end of the third wire; then, with an inſulated diſcharging rod, make a communication from the ball in one bottle to the ball in the other: if the outſide of the firſt phial be negative at the time it is charging, the inſide of the ſecond would be the ſame, and making the above communication would produce an exploſion, and both bottles would be diſcharged, but the contrary will happen, for there will be no exploſion, nor will either of the bottles be diſcharged, although there be a compleat communication between their outſides, becauſe the inſide of them both will be poſitive. This is a proof, that conſidering one ſide of a phial to be poſitive and the other negative, at the time they are charging, is a miſtake: as well as that, if any number of bottles be ſuſpended at the tail of each other all the intermediate ſurfaces, or ſides, do not continue ſo.

EXPERIMENT IV.

Here alſo let the apparatus be diſpoſed as in the laſt experiment, till the bottles are highly charged: then, with a clean ſtick of glaſs, or the like, remove the communication between the ball of the firſt phial and the prime conductor before the machine ceaſes working; then, with an inſulated diſcharging rod, make a communication from the outſide to the inſide of the firſt phial; a ſtrong exploſion will take place on account of the exceſs within-ſide, notwithſtanding they are both poſitive: for as it is expreſſed in page 63, the inſide of the phial poſſeſſed a much greater quantity than its outſide, contrary to Mr. Bewly's obſervation, ‘ *it may be ſaid there ought to* ‘ *be no exploſion between P and p, becauſe they* ‘ *are both poſitive.*’

EXPERIMENT V.

This experiment being ſomething of a continuation of the preceeding one; immediately after the laſt explosion takes place, diſcharge the prime conductor of its electricity and atmoſphere; then touch the ball in the firſt phial with the hand, or any conducting ſubſtance that is not inſulated; then will the inſide coating of the firſt phial, which at firſt was ſo ſtrongly poſitive, be in the ſame ſtate as the outſide coating of the ſecond, having a communication, by the hand, the floor, &c. with each other; that is, negative, if any thing can properly be called negative, or poſitive, that has a communication with the common ſtock: but a pair of cork balls that are electrified either plus, or minus, will no more be attracted by either the inſide coating of the firſt phial, or the outſide coating of the ſecond, then they will by the table on which they ſtand, or a common chair in the room, while they continue in that ſituation. Remove the aforeſaid communication from the ball of the firſt phial; touch the ball in the ſecond, as before in the firſt, or diſcharge the bottle with the diſcharging rod, and the ball in the firſt bottle will immediately become negative: with a pair of cork balls, electrified negatively, approach the ball in the firſt phial, and they will all repel each other, or, if the cork balls be electrified poſitively, they will be attracted. All theſe circumſtances together ſeem fully to prove what has already been ſaid, not only that the inſide of the firſt phial, which was ſo ſtrongly poſitive, may be altered ſo as to become in the ſame ſtate as the outſide of the ſecond, without diſcharging the phial, or any more working the machine; but that it may be fairly changed, from being poſitively charged to being negatively charged. If a pair of cork balls are now hanged on to the ball on the wire in this phial, by the help of a ſtick of glaſs, they will repel each other, being negatively electrified. Make a communication from the outſide of the bottle to the table, and replace the communication from the prime conductor to the ball in the bottle, then, upon moderately working the machine to charge the bottle, the cork balls will ceaſe to repel each other till they touch, and will ſoon repel each other again by being electrified poſitively. Here the working the machine anew, plainly ſhews that the inſide of the firſt bottle, which was poſitive, was likewiſe changed to negative.

Although Dr. Franklin has been ſo often referred to in ſupport of the opinion that the two ſides of the Leyden Phial are in different ſtates, at the time it is charging, it does not appear any where, in his moſt excellent book on the ſubject, that he takes any notice concerning them at that time; but almoſt invariably ſtays till it be charged, before he ſays any thing about its different ſtates; from which there is ſome appearance, either that he has not been rightly attended to, or that he has been wrongly underſtood, although he writes almoſt in general with very great perſpicuity. In that ſame Book [ſee Page 141.

Edit. 1774] Mr. Colden has a very ſtrong expreſſion, that both its ſides are alike at the time it is charging, in his remarks on the Abbé Nollet's Letters to B. Franklin, Eſq. where the Abbé ſays '*that he can electriſe a hundred* '*men, ſtanding on wax, if they hold hands, and* '*if one of them touch one of theſe ſurfaces (the* '*exterior) with the end of his finger,*' Mr. Colden then immediately ſays, '*This I know he* '*can, while the phial is charging, but after* '*the phial is charged I am as certain he cannot.*' And I cannot ſee how any one can draw ſuch ſatisfactory concluſions from any part of Dr. Franklin's writings, as what thoſe Gentlemen, already recited, as well as various others, ſeem to have done, on this ſubject, for the contrary appears plainly to be the fact.

In making electrical experiments, and in particular thoſe in which the Leyden Phial is concerned (a number of which together compoſe moſt electrical batteries) a method to preſerve the bottles, or jars, from being ſtruck through by the electric charge, is very deſirable; but I do not know that it has hitherto been accompliſhed. The number of them that have been deſtroyed in the foregoing, as well as in many experiments, made long before, have led me to various conjectures to preſerve them: at the ſame time I have been obliged to make uſe of bottles inſtead of open mouthed jars. And as coating the former within-ſide is very troubleſome, it has put me on thinking of ſome method more eaſy, quicker, and equally firm and good, as with the tin-foil. With reſpect to the new method of coating I failed: though ſomething elſe preſented itſelf rather in behalf of the former: therefore introducing the proceſs here will not be of very great uſe; unleſs, in ſaving another the trouble of making uſe of the ſame method, or giving a hint towards the former, ſo as to ſucceed with certainty. My aim was, to find ſomething that ſhould be quick and clean, and not eaſy to come off with the rubbing of wires againſt it, and yet a good conductor. My firſt eſſay was with a cement of pitch, roſin, and wax, melted together; into which, to make it a good conductor, I put a large proportion of finely ſifted braſs filings. When this mixture was cold, I put broken pieces of it into the bottle, and warmed the bottle till it was hot enough to melt the cement in it ſo as to run, and cover the bottle within-ſide, then I coated the outſide with tin-foil as is commonly done, and now it was fit for uſe, or ready to be charged: to which I next proceeded; and I believe I had not made more than four or five turns of the winch before it ſpontaneouſly ſtruck through the glaſs with a very ſmall charge; I then took off the outſide coating, and ſtopped the fracture with ſome of my common cement, after which I put the coating on again; and, in as little time as before, it was ſtruck through again in a different place: and thus I did with this bottle five or ſix times; ſometimes it ſtruck through the cement; but it ſtruck through the glaſs in four different places. This made me conſider what it might be, that facilitated the ſpontaneous ſtriking through the glaſs, and likewiſe what might retard it. I had long before thought, that jars or bottles appeared to be ſtruck through with a much leſs charge, juſt after their being coated, or before they were dry, than when they had been coated long enough for the moiſture to be evaporated from the paſte with which I moſtly lay on the tin-foil; and could only conſider the dry paſte as a kind of mediator between the tin-foil and the glaſs, or in other words, that the moiſture in the paſte, was a better conductor, and more in actual contact with the glaſs, than the paſte itſelf when dry. And the coating the bottles with the heated cement, though long afterward, did not alter my former idea; for it appeared as if the hot cement, with the conducting ſubſtance in it, might be ſtill more in actual contact with the glaſs, than the moiſture in the paſte. On theſe probabilities I had to conſider what might act as a kind of mediator more effectually than the dry paſte, between the glaſs and the tin-foil. It occurred, that common writing-paper, as being neither a good conductor nor inſulator, might be ſerviceable by being firſt paſted ſmoothly to the tin-foil, and left to dry. The paper then being paſted on one ſide, having the tin-foil on the other, I put them on the glaſs together with the tin-foil outward, and rubbed them down ſmooth. This ſucceeded ſo well that I have never ſince had any ſtruck through that were thus done, either common phials, or large bottles which contain near three gallons each, though ſome of the latter have ſtood in the battery in common uſe with the other a long time. And as I have never had one ſtruck through that has been prepared in this way, I am much leſs able at preſent to tell how great a charge they will bear before they are ſtruck through, or whether they will be ſtruck through at all.

In the year 1769, as already obſerved, Mr. James Ferguſon read a courſe of lectures in this city, at which I attended, and thoſe were the firſt I ever ſaw. One of the lectures was on electricity; and his machine ſeemed to me to act conſiderably better than any one I had met with before; and, to increaſe the excitation, he put a piece of ſilk between the rubber and the cylinder, (but it was not made faſt to any thing) and applied ſome amalgam, on a piece of ſpunge, to the cylinder. The advantage gained by it ſeemed to me to be very conſiderable; inſomuch that I could not ſatisfy myſelf without applying the like to my own machine, which was ſoon done, but as it was not faſtened to any thing, it was troubleſome by ſliding away. In order to prevent this, I made the ſilk faſt to the rubber, by which I found I not only got rid of the trouble of frequently having to replace the ſilk, but I got more fire: this put me on fixing the ſilk in different poſitions, and in different ſhapes and ſizes, and in a ſhort time I got it to as great perfection as it has acquired ever ſince. At that time by its being entirely new, as to the quantity of electric fire it enabled an operator to produce, many people, from different parts of the kingdom, called on me to ſee it, by which means the uſe of the ſilk ſoon became pretty general, and has continued ſo ever ſince, without any material alteration.

Although I had been converſant in electricity from the year 1749 to 1770, I never ſaw any machine that would produce the quantity of electric fire that my own, by the help of the ſilk, would then do; nor could I ever before charge jars, or phials near ſo high: and if a jar, or phial, at any time diſcharged of itſelf, or ſpontaneouſly, it was attributed to the excellency of the machine. But after I had ſo advantageouſly applied the ſilk, upon making electrical experiments very frequently, I found that the ſpontaneous diſcharging of a phial, &c. depended much more on another circumſtance, that is, their being very clean and dry; and that if the phial or jar, had been warmed (which ſeldom uſed to be done) to make it ſtill more dry and clean, the ſpontaneous diſcharging of it was very conſiderably facilitated: inſomuch that it was very obvious, that a jar, or the like, would not take ſo great a charge when quite clean and dry as it was otherwiſe capable of being made to take; the report of the diſcharge, as well as the time it took to be charged, were ſo very different, that the fact could not eaſily be doubted.

Although the difference of the charge appeared ſo very plainly, even to a by-ſtander, to be very great, yet I could not tell what that difference was, till I contrived my new electrometer; which enabled me to tell the difference very exactly, and very eaſily.

A common pound phial of green glaſs, that contained about thirty-eight ſquare inches of coated ſurface, which I then had, when perfectly dry and clean as I could make it, would diſcharge ſpontaneouſly with a charge of about fourteen or fifteen grains of repulſion; but I found I could alter it, with reſpect to its cleanneſs, ſo as to take a charge whoſe repulſive force ſhould be equal to twenty-four grains.

In a jar that I then had, which contained about ſixty-four ſquare inches of coating, the jar being ſeven inches high and four in diameter, I found the difference much greater; for, when quite dry and clean as I could make it, it would diſcharge ſpontaneouſly with five and a half grains; but by altering its cleanneſs, that is, by dirtying the naked part of the glaſs*, I made it take a charge of thirty-five grains, which is ſix times as much as when quite clean: and it would not then diſcharge of itſelf, but ran over the top, all round the jar, as water would have done by being over filled, and is very conſpicuous in a dark room.

In regard to the application of the ſilk, I owe all hints towards it, to Mr. Ferguſon: for I had not ſeen, nor did ſee, till ſix months after, either Dr. Franklin's Book on, or Dr. Prieſtley's Hiſtory of, Electricity; where I then found that the former had tried a piece of leather ſomething in the ſame way, but he did not make much of it.

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* The Rev. Mr. Morgan having told Mr. Nairne of the great difference of charge, which I had found, that a bottle would take, when the uncoated part of the glaſs was a little dirtied, or not quite clean, Mr. Nairne, ſomewhat facetiouſly ſaid, that I was fortunately ſituated, for it was only the *dirt of Norwich* that would do ſo. Mr. Nairne ſeems to me to be one of the beſt electricians that I have met with.

From the many electrical experiments that I was making in the year 1771, it plainly appeared, that a certain quantity of ſurface diſpoſed in length, and joined to a prime conductor, added much more to the ſtrength, or pungency of a ſpark, or ſtroke, than the ſame quantity of ſurface diſpoſed in thickneſs, or breadth, and joined to the ſame prime conductor.

I had an iron rod five feet long and about three-eighths of an inch thick, with a ball three inches diameter at one end; the other end of it was held in the prime conductor: I likewiſe had a conductor, made of paſte-boards, and covered with tin-foil, about nine inches diameter and twenty-five inches long; but the latter, being joined to the prime conductor, gave a ſpark which was by no means equal to the former, although the difference of the quantity of their ſurfaces was ſo very great. If the hand were ſhut cloſe, and a ſpark were taken on the back of the hand from the prime conductor, having the iron rod joined to it, the whole hand would be convulſed, and if I ſtood on a ground floor, I felt every ſpark, or ſtroke in my feet. I mentioned this to Mr. Bewly, May 20, 1771, but he gave me for anſwer, that he ſomewhat doubted the fact, and thus this part of the buſineſs was then dropped.

Some time in or before the year 1778, the Rev. Mr. Morgan read ſome Italian publications to me; one of which contained an account, given by Sig. Volta, of a number of *Baſtions*, as he called them, or rods, eight feet long and half an inch thick, inſulatedly ſuſpended, in different poſitions; and finding this had ſome reſemblance to my former obſervations, I reaſſumed the ſubject.

Having by me a round iron rod near ten feet long, and fifteen eighths of an inch in circumference; I made another rod, of fir, the ſize and length of the iron one, and covered it with tin-foil. A ball about three inches diameter being put on at each end of the rods, made each rod, with its balls, ten feet long. Theſe were tried ſeparately, to ſee if any ſenſible difference could be diſcovered in the iron rod, from that of fir covered with tin-foil: but one appeared to give a ſpark, or ſtroke, equally ſtrong as the other. I next tried them together, one joined to the end of the other; thus they gave a much more powerful ſtroke. Having theſe two rods ſeparately inſulated, I next placed them parallel to each other, at the diſtance of between three and four feet, and made a communication between them, to obſerve the ſtrength of the ſtroke they would give in that poſition. After this they were removed to about ten or twelve inches diſtance, in order to examine if there were any perceptible difference in the ſtrength of the ſtroke when they were near, or further off, each other. The former, Sig. Volta ſpeaks of, as a very conſiderable hinderance, by the interference of their atmoſpheres; but I could find no ſenſible alteration.

I think I obſerved ſomething of this kind, done very lately; in Mr. Adams's Book on Electricity; or rather, to ſee if there were any difference between a ſolid body and a hollow one of the ſame external dimenſions, but none was found.

One of my rods contains about two hundred and thirteen ſquare inches of ſurface. I have a prime conductor about four inches diameter and near three feet long, which contains about four hundred and ſixty-eight ſquare inches of ſurface; but the latter, joined to my common prime conductor, does not give ſo powerful a ſpark by much, I mean as to ſenſe, as one of my rods joined to the ſame common prime conductor, although the ſurface is more than double.

After trying a rod of wood and a rod of metal, and finding no ſenſible difference, I made twelve rods, the ſize of the former, of fir, covered with tin-foil, and put a ball on at each end about two inches and a half diameter, which made each rod, with its balls, about five feet and a half long. Having placed my former two rods at the diſtance of between three and four feet, nearly parallel to each other and nearly the ſame height, I laid the twelve ſhort rods nearly equidiſtant from each other upon the two long rods, ſo that they were about ten or twelve inches aſunder. Then, upon making a communication from the prime conductor to any part of the rods, and working the machine to electrify, or rather to charge them, they would give a ſtroke much too ſevere to be often taken: inſomuch that (theſe ſtrokes, quickly taken, ſo affecting the legs and ancles, with a powerful machine, ſuch as Mr. Nairne's large ones are, to charge the rods quick enough withal) I believe they would ſoon bring a very ſtrong man down, if he ſtood on a ground floor.

Theſe rods I ſuſpended by glaſs ſticks about a yard long. About the middle part, from the underſide of the rods, I ſuſpended (on points, in order to move freely) another rod, about ſix feet long, with a ball of lead at one end, and a large cork ball covered with tin-foil at the other end, and a Florence wine flaſk covered with tin-foil likewiſe, and ſuſpended freely cloſe to the cork ball: conſequently the center of gravity was much nearer the leaden ball than the middle of the rod, and the end of the arm with the flaſk, on the oppoſite ſide of the center of gravity, was much the fartheſt off. The whole of this additional apparatus being ſuſpended under the former rods, and the whole charged, or electrified together, the flaſk will be attracted by whatever is neareſt to it, either ſideways, or below it. Let a thunder-houſe be placed any where lower than, and in the reach of, the flaſk, or, inſtead thereof the ſpire of a cathedral in miniature, with an interrupted communication to the ground, and a ball on the top of it, the flaſk will approach the ball and diſcharge its electricity upon it, and beat it down, without any phial or jar to aſſiſt it; but if the ſpire terminates in a point, and the communication to the ground be complete, every thing will be ſafe.

If theſe rods can be ſuppoſed to repreſent a thunder cloud in the air, and the moveable flaſk to repreſent the tail, or an inferior part of the cloud, which may be driven about by the wind, or attracted, if charged with lightning; I ſay, if theſe ſuppoſitions can be admitted, I think this is the moſt natural repreſentation thereof that I have yet heard of: and at the ſame time plainly ſhews, which is to be preferred for the termination of conductors to preſerve buildings in thunder ſtorms, or from lightning, points, or balls.

With regard to excellence in electric machines, as to their conſtruction, and power, I have never ſeen any, of its ſize, equal to *Mr. Nairne's* patent one; and much of the apparatus is incomparable, particularly for medical purpoſes; though at the ſame time, the machine is applicable to every other purpoſe where electricity is concerned.

Whatever may be the ſize or form of an electric machine; it ſeems that the general aim in making it, is to have its conſtruction ſuch, as that it may collect, or produce, the moſt electric fire poſſible with the leaſt labour: but at the ſame time I think it is as indiſpenſably neceſſary, that its conſtruction be ſuch, that after the electric fire is collected, or produced, it may be prevented from being diſperſed, or eſcaping, by the admiſſion of conducting ſubſtances into the atmoſphere of the machine, or the prime conductor. It alſo ſeems to me that for the purpoſe of charging of batteries, a large prime conductor is very diſadvantageous: alſo, that it is highly neceſſary for the battery to be placed at ſuch a diſtance from the machine and the prime conductor, as to be as much as poſſible out of their atmoſpheres: and if the machine and the prime conductor, be large, the operator ought to ſtand inſulated; otherwiſe,

otherwiſe the atmoſphere of a large apparatus will extend ſo far round, that a great deal of the collected electric fire will eſcape, or be diſperſed by him into the floor on which he ſtands.

When that excellent ſet of electrical experiments were made by Mr. Nairne, which are inſerted in the Philoſophical Tranſactions VOL. LXVIII. he ſeems to have been well aware of theſe circumſtances in the conſtruction of his apparatus; and if I am rightly informed, the operator, whilſt he wrought the machine, ſtood inſulated.

Amongſt the various conjectures relative to the cauſe of the *Aurora Borealis*, electricity has been conſidered as a principle agent in producing theſe appearances; and in order to verify theſe conjectures, I have, at various times, even whilſt theſe appearances were the greateſt, put up my inſulated conducting rod, to obſerve whether or not it would be affected: but the cork balls in my room, connected with the rod, ſeemed not to diverge any more at thoſe times than when there were no ſuch appearances, nor indeed to be affected by any electricity at all. More obſervations made at different places by different perſons, may probaby caſt further light upon this affair.

N. B. Reſpecting the degree of foulneſs of the naked part of the glaſs already recited, in order to give the greateſt charge to a Leyden Phial, &c. that it is capable of; it ſeems neceſſary, that ſomething of a very ſlight oilineſs be rubbed over its ſurface, or ſomething, nearly a non-conductor, which will adhere to the glaſs very thinly; ſuch as the perſpiration of one's hand, when it *begins* to be in a ſtate of perſpiration, from exerciſe. If the electrical apparatus be kept in a very warm and dry room, the advantage of the above method of ſoiling the naked part of the glaſs will ſoon appear; but if the apparatus be kept in a cool room, where neither a fire is kept, nor the ſun enters, the aforeſaid method will be of very little uſe. What there is in the compoſition of the electric fluid, that ſome part of it ſhould run over the top of the jar and the jar not diſcharge, and that ſome other part of it ſhould ſtay behind and keep the jar ſo highly charged, as recited in page 101, I cannot even give a hint at: but the running over does not begin till the jar is very highly charged, and then it is attended with a conſiderable noiſe, almoſt like very ſtrong hiſſing.

CHAPTER IV.

OBSERVATIONS ON THE AIR PUMP.

AFTER having ſo much improved my electric machine by the application of the ſilk, it acquired ſo much approbation, that many Gentlemen deſired me to ſend them a new machine on the ſame plan as my own, with apparatus to them; amongſt which was the exhauſted, or luminous flaſk. Theſe flaſks I procured from London, till I had ſome difficulty to get them readily; which brought on ſo much inconvenience, that, as I had no air pump, I determined to make one, of the table kind, which, when compleated, proved to be a very good one. With this I made many luminous flaſks, where the difference of exhauſtion plainly appeared, by the time that they would continue to be luminous; and thoſe that were luminous the longeſt were thoſe that were the moſt exhauſted, conſequently, on ſome occaſions preferable: which made me deſirous of exhauſting them ſtill more, if poſſible.

The accounts given of Mr. Smeaton's Air Pump, put me on enquiring of many Gentlemen, &c. about them, in order, if poſſible, to procure one for trial. One of thoſe Gentlemen was the Rev. G. Walker, F. R. S. who gave me for anſwer, that he had one, and I ſhould have it to try*. Very ſoon after this, he came himſelf from Nottingham, where he then reſided, and brought his pump with him. We took a time for trial of it, together with my own pump; when ſeveral other Gentlemen met us, and we gave them a fair trial: but could find no difference between his and my own, in reſpect to the power of rarifying. When it was in the beſt order we could make it, we could bring the ſame gage, with either pump, down to about four lines and a half, but no lower. By this it plainly appeared, that the Smeatonian Pump was not altogether what it had been repreſented: but they were both uſed with wet leathers. Having found that either pump would do the ſame with the ſame gage, it occurred that either pump might appear to do differently with a different gage, that is, one ſomething like Mr. Smeaton's, or the pear-gage. And having read Mr. Smeaton's account in the Philoſophical Tranſactions, Vol. XLVII, I determined to put his deſcriptions into practice as well as I could: and in order thereto, I made a glaſs ſimilar to his for a gage, but ſomething longer. The tube part of the gage was ſix inches long above the bulb; and the ſize of the bore was ſuch, that the weight of the mercury, which filled three inches length, was five penny-weights, nine grains and a half. I exhauſted this glaſs, over a cup of mercury, ſet upon the air pump, under a common gage glaſs receiver, ſo that all the new gage was above the mercury. When it was exhauſted nearly as far as I could, I let the gage down into the mercury, and opened the pump to fill the gage by the preſſure of the common air. When it was as full as it would fill, I marked the glaſs tube as it ſtood erect (having taken off the receiver) with a file, at the top of the column of mercury. After this, I took out the remaining air at the top of the gage, by the help of a wire, and filled the whole gage as full as I could with mercury: and having emptied the gage, I weighed the mercury which it contained, and found its weight, eight ounces, fourteen penny-weights, and twelve grains. Then I filled that part of the gage which was above the aforeſaid mark (made with a file on the gage) with mercury: and weighing this portion by itſelf (as I thought this a more certain method than truſting to diviſions on the gage) I found that by this new gage, my pump appeared to rarify near two hundred times, and that this gave me a much greater number of times rarifying than my common gage, which I had on the pump at the ſame time. This apparent difference made me deſirous to repeat the experiment, and to be as exact and careful about every particular as I could. Accordingly, I then new valved and leathered my pump, and made the experiment again; and found five hundred and fifty-one times rarefaction.

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* I informed Mr. Bewley of this on the 25th of July, 1776.

The two foregoing experiments falling out in this manner, made me determine to try tubes of various ſizes and lengths, joined to the ſame bulb, one after another. Accordingly, I cut my preſent new gage aſunder at one inch above the bulb; and to this ſame bulb and ſhort part of the tube I joined, with cement, another tube twenty-five inches long. I alſo lengthened the tube part of my gage glaſs, for a receiver; and on proceeding with this as with the former, it gave me ſixty-ſeven times rarefaction. Three inches of this bore contained three penny-weights, thirteen grains, and a half of mercury.

EXPERIMENT III.

I then ſeparated this long tube from the bulb, at the place where they were joined, and to the ſame bulb as before, I joined another tube fourteen inches long; and on repeating the former proceſſes, it gave three hundred and fifty times rarefaction. Three inches of this tube held four penny-weight, two grains, and a half of mercury.

EXPERIMENT IV.

Having ſeparated this fourteen inch tube from the bulb at the juncture, I joined another tube five inches long; ſo that my gage was thus the ſame length as the firſt, but the tube was ſmaller: and on trial as before with the others, this gave eight hundred and ninety-eight times rarefaction. Three inches of this tube contained one penny-weight and twenty-three grains of mercury.

EXPERIMENT V.

I here took the piece of tube which with the ſame bulb made the firſt new gage, and cut it aſunder at two inches from the top of it: this piece of two inches I joined to the aforeſaid bulb; and found by *this*, that my pump rarified nine hundred and two times. The ſize of the tube is given with the firſt trial.

EXPERIMENT VI.

My next trial was with the laſt mentioned tube but one; which I alſo cut aſunder at two inches from the top: and having joined the piece of two inch long to the former bulb, as before, I found that by this ſhort and ſmall tube my pump rarified one thouſand five hundred and ninety-eight times. This is a piece of the tube, of which three inches held one penny-weight, and twenty-three grains of mercury.

The above experiments are numbered, as beginning where and when the pump was new valved, &c. and the fractional parts of rarefying are omitted, as being unneceſſary.

It appears that Mr. Smeaton's gage was not quite four inches long, including the bulb and tube together: and he ſays that the gage contained about half a pound of quickſilver, and that a thouſandth part thereof would fill about one-tenth part of an inch in the tube-part of his gage. On the whole it appears that the tube-part of his gage was much ſhorter than mine; ſo that if my gage had been as ſhort as his, my pump might have appeared to rarefy as many times as his: for the length as well as the ſize ſeems to be of conſiderable conſequence; and more ſo, if I had uſed the method that Mr. Smeaton uſed, that is, to turn the gage horizontally before he took the ſpace between the top of the column of mercury and the top of the tube. Taking the ſpace in his manner, would have indicated a much greater number of times rarefaction, than the manner I uſed (as I found by trial) conſequently much further from the truth.

I think that if the principle, on which this gage is ſuppoſed to act, were well-grounded, the gage would ſpeak the ſame language in all caſes: but that it does not, is very plain, or I am much miſtaken.

The principle on which I ſet out, on the ſubject of rarefaction, is to ſuppoſe a common torricellian tube well filled with mercury, and purged of air as much as poſſible, placed erect in a ciſtern of mercury: let the tube be about thirty-two inches high above the mercury in the ciſtern; and the mercury in the tube will fall to a height, which will be an exact balance to the common atmoſphere: and placing a receiver over the whole upon an air pump, and exhauſting the air out of the receiver, the mercury will ſubſide in the tube ſo as to be always a counterbalance to the preſſure of the air, which acts upon the ſurface of the mercury in the ciſtern. Thus, I think I may ſay, as the ſpace through which the quickſilver in the tube has fallen, is to the ſpace through which it has to fall (to be level with the ſurface of the quickſilver in the ciſtern) ſo is the degree of exhauſtion, or the quantity of air taken out of the receiver, to the quantity remaining in it*.

Mr. Smeaton ſays (ſpeaking of his valve) that in the common pump, '*the valves are* '*ſtretched.*' I believe that if the valves in any pump be properly put on, they will riſe much eaſier than his; their weight, to be raiſed, may be much leſs: and the places of coheſion round the holes may be much leſs: for the valves will lie plain on the *ſides* over a ſmall hole much better than over a larger one, when a power to preſs them in is applied: and when this is the caſe, if the valve ſhould riſe any ſooner, in the middle part, and the air in a rarefied ſtate be not of force enough to raiſe the outermoſt part of it, this being where the greateſt coheſion is, and, as aforeſaid, will not riſe ſo ſoon as a valve in a common pump may be made to do*; the pump may be kept working, and no air will come out, continue the working ever ſo long:

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* If this principle be not a true one, I am deceived, but I believe it will hold good at all times, and in all places. And in all the foregoing experiments, with the pear gage, I believe the utmoſt degree of rarefaction was very little more than fifty-nine times, having another gage in the pump all the time, as before obſerved.

If in a torricellian tube, well purged of air, the mercury ſtands thirty inches high, and if with an air-pump it can be brought down to half an inch of the level of the ſurface of the mercury in the ciſtern in which the tube is placed; I think it may be ſaid, that the mercury is brought down fifty-nine parts out of ſixty; and conſequently, that fifty-nine parts out of ſixty, of the air that was in the receiver on the pump, is taken out; and that the air in the receiver is but one ſixtieth part ſo denſe as the common air; or in other words, that the air in the receiver is rarefied fifty-nine times: and that pump which will bring the gage the loweſt, I ſhould readily allow to be the moſt perfect.

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* To make a valve for an air pump, I ſhould recommend a ſtrip of very thin bladder, about five-eighths of an inch broad, made very pliant by rubbing it in the fingers with oil, then fixed down cloſe as poſſible; by *ſtretching* it over the hole that leads to the bed of the pump; a ſlit to be cut at right angles to the ſtrip on each ſide of the hole, about one-fourth of an inch diſtant from the hole with a ſharp knife, or the like, held a little leaning from the hole, and ſome oil put upon it. By this method I think a valve may be made to riſe the eaſieſt, and be the moſt effectual of any that I know of. Thus, inſtead of hindering, the *ſtretching*, is very ſerviceable: or in the manner of Mr. Bewly's pump made by Mr. Nairne, where four ſlits may be cut: in either, the valve will be held by four places, as well as Mr. Smeaton's, though much more eaſily raiſed.

therefore, how can any advantage be expected in or from the valve?

Having informed Mr. Bewly, that I was to have a Smeatonian Pump ſent me for the purpoſe of trying it, he wrote to me on the 17th of Auguſt 1776, as follows: ‘ When you ‘ laſt wrote to me you told me you were ‘ going to *review* an air pump of Smeaton's ‘ conſtruction. I ſhall be much obliged to ‘ you if you will inform me of the reſult ‘ of your examination.’ Accordingly on the 19th of the ſame month I ſent the reſult to the purport as in pages 114 and 115. Alſo in the ſame letter I wrote to him thus: ‘ If Dr. ‘ Prieſtley would ſend his pump to Nor- ‘ wich for us to try, I would willingly pay ‘ the carriage hither and back again. Do you ‘ think it would be a fair queſtion to aſk him? ‘ Such properties as he ſpeaks of are very de- ‘ ſirable in a pump; but if he is under any ‘ miſtake about it, I ſhould think he would ‘ like very well to be ſet right: let me aſk the ‘ favour of your anſwer.’

Mr. Bewly did not write to me any more till the 31ſt of Auguſt, 1776. But on the 27th preceding, as I had been making further trials, I wrote to him again, informing him, 'that I thought I had found out the myſtery 'by which Mr. Smeaton's pump and gage in'dicated ſuch great powers and degrees of ra'refaction: and that with my own pump and 'his gage I could, without any ſort of diffi'culty, rarify fifteen hundred times, and much 'more.' Alſo I informed him, in brief, of ſome reſults which inclined me to think ſo; and that, if it were worth his knowing, I would ſend him the particulars.

On the 31ſt of Auguſt 1776, as before obſerved, Mr. Bewly wrote to me again, as follows, 'I am much obliged to you for the in'formation I received by your letter, rela'tive to the Smeatonian gage. Though you 'ſay nothing of the cauſe of the deception you 'have diſcovered; yet I think I can make a to'lerable gueſs at it.' He likewiſe tells me that, 'another Gentleman* with himſelf had juſt 'been talking of this gage. It occurred that, 'on the mercury's riſing into the gage, it muſt 'probably entangle a great part of the in'cluded rarified air, particularly againſt the 'ſides of the glaſs; ſo that the air which ap'pears in the upper part of the tube, and 'which Smeaton conſiders as being the *whole* 'that had been left in the gage, on the ex'hauſtion, may only be a tenth part of it; 'the remainder being diſperſed over the inner 'ſurface of the large globular part of the 'gage. Pray inform me whether this is not 'the caſe. I ſhould be glad likewiſe to re'ceive the particulars.'

Though Mr. Bewly had no opportunity to make his conjectures perfectly ſatisfactory; yet I think it will appear in the ſequel, that they were well founded.

In the ſame letter he adds, in very ſtrong terms, tending almoſt to a degree of wonder, or in the height of ſurpriſe, ‘The only difficulty that ſtrikes me, is how Mr. Smeaton ‘and Dr. Prieſtley could deceive themſelves ſo ‘egregiouſly in this matter. Did they never ‘uſe the *common gage* along with this? If they ‘had, and if the pump had actually rarefied ‘the air ſometimes even 2000 *times*, or even ‘1000, the mercury in the common gage muſt ‘have been brought down to a level,—I mean ‘as to ſenſe, with that in the ciſtern. I do not ‘find however that they any where aſſert this. ‘Could they wilfully ſhut their eyes, and believe the air was rarefied to 2000, when the ‘mercury was at the ſame time ſtanding in the ‘common gage at the height of three or four ‘lines at leaſt? I know not how to reconcile ‘theſe things.

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* The Rev. Mr. Brand.

‘Not two hours before I received your letter I had ſent one off to Dr. Prieſtley, in ‘which I had informed him of the reſults of ‘your examination of a Smeatonian pump, and of ‘your doubts and difficulties. I wiſh I had had ‘your letter before this was ſent off; but if you

‘ will enable me to be more particular, I will ‘ give him a full account of the matter.’

After theſe requeſts of Mr. Bewly's, I ſent him the particulars on the 9th of September 1776, nearly as recited in the laſt ſix Experiments, either for Dr. Prieſtley's, or his own uſe; to which he gave me an anſwer on the 21ſt of September 1776, In the following words. ‘ I return you many thanks for your gages,’ [I had previous to this, ſent Mr. Bewly ſeveral pear-gages,] ‘ and your epiſtolary communications, ‘ which ſatisfactorily prove the fallacy of the ‘ *Smeatonian gage*; though you will find below, ‘ that Dr. Prieſtley ſpeaks with confidence ‘ of the great powers of the pump of that ‘ name.’

‘ When I wrote firſt to Dr. Prieſtley on the ‘ ſubject of Smeaton's Pump, I could only ‘ inform him, in general that you had found it ‘ in no reſpect ſuperior to a good common ‘ pump, in the article of rarefying. Here is ‘ what he ſays on the ſubject.’

“ *The pump which Mr. Brook examined muſt* “ *have been a very bad one, indeed. Mr. Nairne* “ *has improved upon Mr. Smeaton. Before I left* “ *London, I ſaw one of his exhauſt near ten thou*“*ſand times**.”

‘ Surely, all theſe good people, though they ‘ may make a miſtake of a few thouſands in ‘ eſtimating the power of the Smeatonian ‘ pumps, in particular, muſt be able at leaſt to ‘ rarify a few hundreds.

‘ Since you informed me of your experi‘ments made with Smeaton’s gage, I have, in ‘ general, informed Dr. Prieſtley of the reſults, ‘ and have put ſome queſtions to him on the ‘ ſubject. As ſoon as I hear from him, I will ‘ communicate to you his anſwer, or am in‘formed whether theſe potent rarefiers of air, ‘ to 10,000 and upwards, have any other colla‘teral evidence to produce, to ſtrengthen the ‘ ſuſpicious teſtimony of the Smeatonian gage.’

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* Mr. Nairne is one of the beſt Philoſophical Inſtrument-makers I ever knew.

Hitherto Dr. Prieſtley appears to be pretty firm in his opinion, that the Smeatonian apparatus has great ſuperiority, notwithſtanding what Mr. Bewly had informed him of: but now he ſoon finds his miſtake.

On the 7th of October 1776, Mr. Bewly ſent me another letter, in which he writes as follows: 'I received a letter from Dr. Prieſt-'ley laſt poſt: the contents of which, ſo far 'as they relate to the Smeatonian pump and 'gage, you will be curious to know.

'In a former letter he had told me that he 'never had uſed a gage in which the mercury 'had been boiled; and that he knew not how 'to execute the proceſs. I deſcribed it to him 'in the moſt minute and circumſtantial man-'ner; deſiring him to prepare a ſimple gage 'after this manner, and then try whether his '*Smeaton's pump* would bring the mercury con-'tained in it within a *line*, or leſs, of the mer-'cury in the baſon; as it ought to do, if it 'rarefied the air *only* 3 or 400 times. He has

‘ tried the experiment; and the following is the ‘ reſult:

‘ His pump was foul, not having been ‘ cleaned for ſeveral months paſt; “ *and his* “ *common ſyphon-gage ſhewed that it did not ex-* “ *hauſt near ſo much as it had formerly done.*” ‘ He was content with it nevertheleſs, and in ‘ this ſtate, he included both the Smeatonian ‘ gage and the boiled gage in the receiver, at ‘ the ſame time. On exhauſting, Smeaton’s ‘ gage gave him 300 degrees of rarefying ‘ power; while the mercury in the boiled gage ‘ fell down only to a little leſs than *half an* ‘ *inch* above the level; and accordingly indi- ‘ cated only about 60 degrees. He would ‘ have cleaned his pump, he ſays, had he not ‘ been convinced that what he had already ‘ ſeen was ſufficient to diſcredit the Smeato- ‘ nian gage. He obſerves that “ *you will gain* “ *great credit by the detection;*” and that the gage ‘ is certainly fallacious, though he does not ‘ comprehend the cauſe of the fallacy.’

Notwithſtanding Dr. Prieſtley already appears to be ſo perfectly convinced, that the Smeatonian pump and gage were not what they were ſuppoſed to be; he does not content himſelf with informing only Mr. Bewly of it, but, with his uncommon frankneſs, without ſtaying any longer, on the 20th of October 1776, he writes immediately to me as follows: ‘ You have great merit in the de‑
‘ tection of the fallacy of Mr. Smeaton’s gage:
‘ and I wiſh you would draw up a regular ac‑
‘ count of your obſervations on that ſubject.
‘ It is of great uſe to know what we have to
‘ depend upon in buſineſs of ſuch importance
‘ in philoſophy. *Mr. Nairne*, who makes the
‘ beſt pumps of Mr. Smeaton’s conſtruction,
‘ has heard of your objections to them, and
‘ will ſoon, I believe, give Mr. Bewly an ac‑
‘ count of what may with certainty be ex‑
‘ pected of them. I intend ſoon to write to
‘ Mr. Smeaton himſelf on the ſubject, and
‘ you ſhall know what he ſays if I have any
‘ anſwer.’

Accordingly I received a letter from Mr. Bewly, dated November 24, 1776, in which he writes as follows: ‘ By a letter received from ‘ *Mr. Nairne* yeſterday, I find he is to ſend me ‘ next week a pump with which he has taken ‘ particular pains*, accompanied with an ac- ‘ count of ſome experiments relative to the ‘ ſubject of exhauſting. Such experiments as ‘ I can make with my preſent inſtruments, I ‘ will take an early opportunity of making, ‘ and inform you of the reſults.’

The above-mentioned account of ſome experiments is dated Nov. 22, 1776, which by *Mr. Nairne's* permiſſion, Mr. Bewly favoured me with to peruſe, on the 30th of January, 1777; and they appear to be the firſt that *Mr. Nairne* had communicated to Mr. Bewly, and likewiſe ſome of the firſt that he had made on this part of the buſineſs, as will more fully appear afterwards.

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* Mr. Bewly afterwards ſent me this pump to try; and I pronounced it one of the beſt I ever ſaw.

Mr. Nairne ſays, in a letter to Mr. Bewly, dated Auguſt 15, 1777, ‘I cannot conceive ‘why Mr. Brook ſhould think that my trials ‘originated from any experiments I had heard ‘of his, either by his letter to Dr. Watſon or ‘yours to me. If he has Mr. Smeaton’s letter ‘to Dr. Watſon in anſwer to his, he will find, ‘I ſhewed Mr. Smeaton, Hon. Mr. Cavendiſh, ‘Mr. Aubert, and ſome other Gentlemen, the ‘great difference between the two gages, &c. ‘in April 1776, (viz. above 2000 times by ‘the pear-gage) whereas his letter to Dr. ‘Watſon is dated Nov. 16, 1776*, and your ‘letter to me is dated Sept. 7, 1776, wherein ‘you firſt mentioned Mr. Brook. Before I ‘received that letter of yours I had never ‘heard that there was ſuch a perſon as Mr. ‘Brook, that made air pumps or that tried experiments on that inſtrument.’

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* My letter to Dr. Watſon was ſelected from the experiments I made immediately after the receipt of Mr. Bewly’s letter, dated Aug. 31, 1776, and communicated to him Sept. 9, following. But Mr. Nairne ſeems here to be only preparing to make thoſe experiments which were communicated to Mr. Bewly on the 22d of Nov. 1776, in conſequence of his having heard of *my objections*, according to Dr. Prieſtley, ſee page 132.

Hence it appears, as if Mr. Nairne were deſirous to date the beginning of his trials to find out the deception of the Smeatonian gage, from the time he firſt diſcovered the difference between the two gages, which was in April 1776. But though he then diſcovered a difference between the two gages, it does not appear that he did any thing more about it, till he made the experiments which he gave an account of to Mr. Bewly on the 22d of November, 1776: but that all the time from April, 1776, till about the time Dr. Prieſtley informed me that Mr. Nairne had heard of my objections, the buſineſs lay dormant. But neither then, nor afterwards, does it appear, that either himſelf, or Dr. Prieſtley, found, or even ſuppoſed, any deception in the Smeatonian pump as well as the gage: whereas it was the deception of the pump alone, that firſt occurred to me, not having any pear-gages to try. But it plainly appeared to me, before the 19th of July, 1776, that the pump was fallacious [ſee pages 114 and 115] and alſo that I had diſcovered the gage to be equally fallacious as the pump, before the 27th of Auguſt, 1776, and that I communicated an account of both to Mr. Bewly at the times already ſpecified. Likewiſe it is plain, that Mr. Bewly gave an account thereof to Dr. Prieſtley on the 31ſt of Auguſt, 1776. And it is not leſs plain, that Dr. Prieſtley was well ſatisfied, that there was a fallacy, before the 7th of October, 1776, and that Mr. Nairne, *had heard* of my objections to them before the 20th of October, 1776, and about that time ſet about thoſe experiments dated Nov. 22, 1776.

Mr. Nairne's accounts and experiments are printed in the Philoſophical Tranſactions, Vol. LXVII. But if any claim is to be laid to the diſcovery of the fallacy of either the Smeatonian pump or the pear-gage, I think I have the greateſt: the former of which he does not ſeem even now to ſuppoſe. However, I think it will plainly appear in the ſequel, that any tolerably good common conſtructed air pump may be made *to appear* to exhauſt equal to any on Mr. Smeaton's conſtruction,

tion, with the ſame gage to both; conſequently that the ſuppoſition relative to the excellency of the conſtruction of his pump in the article of rarefying, is alſo fallacious.

Mr. Smeaton ſpeaks of his conſtruction, as being eaſier to fit the bottom of the piſton cloſe to the place where the valve is fixed, that leads to the bed of the pump. But I think this ſuppoſition is not leſs fallacious than the ſuppoſition of the excellency of the whole pump, particularly in this reſpect. For I can ſee no cauſe, or reaſon, why the piſton cannot be made to fit as cloſe at the bottom of it, in a common conſtructed pump, as it is in one of his conſtruction: I can ſee, or find, no difference in either. If I could ſuppoſe any, it would be in his, as being the largeſt in diameter, and not leſs difficult to fix at right angles to his rod. In ſhort, the only circumſtance of uſe in Mr. Smeaton's pump, by which any convenience or advantage is gained over the common conſtruction, is *condenſing;* all the other advantages being only ſuppoſitions. Indeed, I think ſuch as Mr. Nairne makes, of the table kind, are very much preferable.

Mr. Smeaton ſpeaks of uſing water with his pump; not then knowing how much it favoured the deception of the pear-gage: which will plainly appear afterwards.

On the 16th of December, 1776, Mr. Bewly ſent me an abſtract of Mr. Nairne's account of ſome experiments, dated the 22d of November, 1776, in which the pear-gage was uſed with the common one. This put me on making more pear-gages, (ſix) with which I made many experiments, and repeated them many times: but, as they were not very materially different from Mr. Nairne's, I ſhall inſert only three of them.

EXPERIMENT I.

A pear-gage, the tube of which was ſix inches above the bulb, three inches of it containing ſix penny-weights two grains and a half of mercury, and the whole gage containing ſeventeen ounces, ten penny-weights, eleven grains, with the apparatus all dry, indicated one hundred and twenty-ſeven times rarefaction, with or without box-wood* in the receiver.

EXPERIMENT II.

A pear-gage, whoſe tube was three inches long above the bulb, and held one penny-weight and twenty-three grains, and the whole gage eight ounces, ſix penny-weights, and ſeventeen grains of mercury, with dry apparatus, gave two hundred and four times rarefaction, with or without box-wood, weighing one hundred and ſeventeen grains.

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* Mr. Nairne found that a piece of box-wood, weighing one hundred grains, included with the pear-gage in the receiver, made a great difference: but I could find none, though cut from different logs, but all pretty dry.

EXPERIMENT III.

The laſt gage, under the ſame receiver, (the receiver, inſtead of being cemented to the bed of the pump, being ſet upon a piece of wet leather) indicated one thouſand three hundred and thirty-three times rarefaction.

Here the effect of the water, in regard to the deception of the Smeatonian gage, is pretty obvious, [ſee page 138.] It is not leſs obvious that a common pump may be made to rarefy as much as a Smeatonian one will [ſee page 136] though the apparent difference of rarefying is above eleven hundred times.

In order that there might be no moiſture in the way, previous to theſe three experiments, I cleaned and refitted my air pump, at the ſame time warming it by the fire, that the oil and valves might be as thin and pliant as poſſible; ſo that I could bring the boiled gage down to five-ſixteenths of an inch; which, if the barometrical weight of the atmoſphere be equal to a column of mercury thirty inches high, indicated a rarefaction of ninety-ſix times. I likewiſe made two new plates to my air pump, of thick plate glaſs; one for the bed of the pump, and the other to ſet the gage upon: and inſtead of uſing wet leathers to ſet the receivers upon, I cemented them down to the plates of glaſs, with a cement of turpentine, bees-wax, and tallow, with a little red lead to colour it; ſo that the pump was uſed without any water, or wet leathers.

Theſe experiments, together with thoſe not here inſerted, which I noted down, amounting to about forty, were made after December 16, 1776, and communicated to Mr. Bewly, Jan. 30, 1777.

The following is a ſet of experiments with different Smeatonian, or pear-gages, in which the mercury was *boiled* in every part of them when ſo ſpecified.

EXPERIMENT I.

A pear-gage, the tube part of which was ſix inches and three quarters above the bulb, three inches of the bore holding five pennyweights and nineteen grains, and the whole gage containing fifteen ounces, ten pennyweights, and twenty-three grains of mercury, gave two hundred and ninety-nine times rarefaction. This was boiled before I began the experiment.

EXPERIMENT II.

The laſt gage new filled and boiled, and with a piece of ſheep ſkin dreſſed with allum, which weighed one hundred and ten grains, incloſed in the receiver with the gage, now gave one thouſand, eight hundred, and ſixty-five times rarefaction.

EXPERIMENT III.

The ſame gage as before, unboiled, and without the ſheep ſkin, now gave one hundred and ſeventy-five times rarefaction.

EXPERIMENT IV.

The former gage, not boiled, but with the ſheep ſkin, now gave fourteen thouſand nine hundred and twenty-two times rarefaction.

EXPERIMENT V.

With a pear-gage, the tube part of which was five inches above the bulb, and three inches of the tube held three penny-weights and two grains of mercury, and the whole gage contained ſix ounces, eighteen penny-weights, and thirteen grains. This gave four hundred and ſeventy-ſeven times rarefaction, unboiled and without the ſheep ſkin.

EXPERIMENT VI.

The laſt gage, with the ſheep ſkin, and not boiled, but without any other known difference, now gave a number of times rarefaction not to be aſcertained with any preciſion whether twenty, fifty, or an hundred, thouſand times, and with as much propriety one as the other; for the ſpace left vacant did not appear to be larger than about half the ſize of a muſtard-ſeed; though the upright boiled gage was not ſenſibly different from all the former, which was each time brought down to three tenths of an inch, and indicated one hundred times rarefaction.

Theſe experiments ſeem more fully to prove, that a common pump may be made to rarefy as much as any one of the Smeatonian conſtruction, as already recited.

EXPERIMENT VII.

In the time that I was making the foregoing experiments, I tried many other pear-gages in which I boiled the mercury: but as the reſults of many of them were ſimilar to the firſt two, I did not note them down. Many of them were ſo well boiled, that the mercury was ſuſpended to the top of the tube at different diſtances: and notwithſtanding I uſed violence to ſhake it down, in ſome of them the coheſion was ſo great, that I could not get it down at all till I opened the pump.

Although it was contrary to my inclination, not to get all the mercury down in the tube, I am now inclined to think that this was a favourable part of the buſineſs. And although the upper part of the column could not be ſhaken down, yet it would not materially alter the experiment. For in all theſe experiments with the boiled pear-gages, I inverted the open end of them into a ciſtern of mercury before I began to exhauſt: and when the pump was exhauſted as much as I could, I raiſed the open end of the gage out of the mercury in the ciſtern, to let out the mercury in the gage, and to let the rarefied air enter: and then nearly the ſame ſpace between the two ends of the columns of mercury in the tube would be unoccupied, as if the whole column had come down, and the unoccupied ſpace had been at the top of the tube.

After the gage was filled in the Smeatonian manner, I took out the air between the two columns, and let them join together; after which, although the open end of the gage was not lifted out of the mercury, till the pump was exhauſted as much as I could exhauſt it, and then only raiſing it out and puſhing it down into the mercury again as quick as I could, there was ſo much vapour or damp, &c. that had taken place on the inner ſurface of the tube, that after I had taken the gage out of the receiver, the place of the juncture was viſible to the greateſt exactneſs; whereas had the whole column of mercury came down at firſt, I ſhould not have been able to diſcover this coating of vapour, &c. on the inſide of the tube.

EXPERIMENT VIII.

I went on to try ſeveral gages in this manner, that had a part of the mercury remain, or ſuſpended, at the top of the tube, and always found this covering with damp, or the like, to take place, when the pump was exhauſted as much as I could, in gages that had been well boiled: and had they not been boiled, none of the mercury would have remained at the top of the tube.

EXPERIMENT

EXPERIMENT IX.

Proceeding on a ſuppoſition that I might not ſo perfectly boil the mercury in the bulb part of the gage as in the tube part thereof; and that if any air, &c. was left in any part, I might diſcover it by exhauſting the receiver to let the mercury ſubſide into the ciſtern out of both tube and bulb, and letting it up again, without taking the open end of the gage out of the mercury (this gage alſo having part of the column ſuſpended at the top) by letting the mercury up into the gage to fill it as it ſtood, only a very ſmall ſpeck was diſcoverable at the joining of the mercury in the tube: ſo that the bulb, as well as the tube, which together held nine ounces and ſeventeen penny-weights, appear to have been very well boiled.

EXPERIMENT X.

Finding no damp, &c. to appear in the tube by filling the gage under the receiver, after the mercury had been let down, and letting in the air to fill the gage by its preſſure, without taking the open end of the gage out of the mercury; my next trial was with a pear gage, the tube of which was five inches above the bulb, to ſee if there would be any damp, &c. lodge upon the inner ſurface of the gage, by emptying part of it and filling it again in open air: and having boiled the gage, I exhauſted the receiver, with the gage in it, to let the mercury ſubſide out of the gage into the ciſtern; and then filled the gage again by the preſſure of the air, without taking the open end of the gage out of the mercury. This, alſo, having part of the column ſuſpended at the top of the tube, as before, exhibited the aforeſaid ſpeck at about half an inch from the top. In this I took the gage out of the receiver, and emptied the bulb and about half the tube, and filled it again in open air: but now not the leaſt more of this coating, &c. appeared in the tube than what appears in every clean tube that has been filled with mercury and not boiled. I repeated this often; and the reſult was the ſame.

Theſe appearances, I think, ſeem ſtrongly to indicate, that this coating, or covering, with damp, or vapour, &c. takes place only where the air is in a rarefied ſtate, or at leaſt, moſt when the air is highly rarefied. I believe it is generally allowed, that water or damp abſorbs air very copiouſly. Thus there may be no great difficulty to ſuppoſe the aforeſaid coating, or covering with damp, vapour, or the like, to do the ſame: and admitting this, the difficulty of accounting for the irregularities and deceptions of the pear-gage ceaſes at once. In this caſe alſo, not only the abſorbency of the damp, &c. may be ſo favourable to the deception in the pear-gage; but if any of theſe tubes thus covered on the inſide with damp, &c. are examined with a magnifying glaſs, they will appear like any other glaſs that has been breathed upon, affording numberleſs vacuities and lodgments in which the mercury will entangle the rarefied air all the way as it riſes in the gage. This verifies

Mr. Bewly's ſuggeſtions in a very high degree*. [See page 126.]

Notwithſtanding all this difference in the pear-gage, the upright boiled gage was not ſenſibly affected, which was each time brought down to three-tenths of an inch. And I believe this to be the moſt accurate meaſurer of air rarefied, or condenſed, to any degree of which the apparatus is capable.

In the abſtract of Mr. Nairne's letter which Mr. Bewly ſent me on the 16th of December, 1776, he mentioned a great difference that Mr. Nairne found, by including a piece of box-wood in the receiver with the pear-gage: after which I made ſome experiments including a piece of box-wood with the pear-gage in the receiver; but found no difference.

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* The Honourable Mr. Cavendiſh ſeemed to have formed an idea ſomething ſimilar to Mr. Bewly's, as related in Mr. Smeaton's anſwer to my letter to Dr. Watſon: which is noticed afterwards.

[See note, page 139.] However, after I informed Mr. Bewly of it, he wrote, by way of note in a letter, dated January 29, 1777, as follows: ‘Though you made your apparatus as ‘clean and dry as poſſible; yet you could not ‘probably clear it of the humidity contained ‘in the inner works of it. If ſo, there might ‘be no alteration, whether the box-wood were ‘under the receiver or not.’ I think it ſeems as if he thought my pump did not rarefy far enough to bring on the difference with the box-wood; which put me on making ſome more experiments with the pear-gage, to ſee what degree of exhauſtion is neceſſary to make a difference appear: and in order thereto, I made two new pear-gages of very different ſizes.

EXPERIMENT XI.

One of the above gages, the tube part of it being three inches above the bulb, held ſeven penny-weights and fifteen grains of mercury; and the whole gage contained fifteen ounces, eighteen penny-weights, and fifteen grains: having boiled the mercury in it, but without box-wood or ſheep-ſkin, on exhauſtion it gave two hundred and fifty-four times rarefaction; and the upright boiled gage was brought down to three-tenths of an inch.

EXPERIMENT XII.

The ſame gage not boiled, with a piece of ſheep-ſkin, which weighed one hundred and twelve grains, included in the receiver, now gave fifteen thouſand, two hundred, and ninety-four times rarefaction: and the upright gage was three-tenths of an inch as before.

EXPERIMENT XIII.

The ſame gage again, not boiled, and without the ſheep-ſkin, gave ſeventy-eight times; the upright gage being brought down to five-tenths of an inch.

EXPERIMENT XIV.

The ſame pear-gage with a piece of ſheep's-leather, gave three hundred and ſixty-four times; and the upright gage was at five-tenths of an inch, as in the laſt experiment.

EXPERIMENT XV.

The laſt gage again, without the ſheep's leather, gave forty-five times; the upright gage being brought down only to eight-tenths of an inch.

EXPERIMENT XVI.

The former gage again, with the piece of ſheep's leather, gave ſixty-ſix times; when the upright gage was at eight-tenths.

EXPERIMENT XVII.

With another pear-gage, the tube part of which was five and a half inches above the bulb, three inches of it held ſix penny-weights and twenty-one grains, and the whole gage contained ſeven ounces, nineteen penny-weights, and twenty-two grains. This gage being filled, and the mercury boiled in it, without the ſheep's ſkin, gave one hundred and ninety times. The upright gage was brought down to three-tenths of an inch.

EXPERIMENT XVIII.

The laſt gage, not boiled, and with the former piece of ſheep's ſkin, the upright gage being brought down to three-tenths of an inch, the former gave fourteen thouſand times rarefaction.

EXPERIMENT XIX.

Bringing the upright gage down to five-tenths, the laſt pear-gage unboiled, and without the ſheep's ſkin, gave ſixty-nine times rarefaction.

EXPERIMENT XX.

Bringing the upright gage down to five-tenths, the laſt pear-gage, with the ſheep's ſkin, and unboiled, now gave one hundred and ninety-one times rarefaction.

EXPERIMENT XXI.

The laſt pear-gage again, unboiled and without the ſheep's ſkin, gave only thirty-nine times, when the upright gage was brought down to only eight-tenths of an inch.

EXPERIMENT XXII.

The ſame gage again, unboiled, and with the piece of ſheep's ſkin, the upright gage being at eight-tenths, gave fifty-ſeven times rarefaction.

In all theſe experiments, the pump was uſed dry, and the receivers were cemented down; and in all the firſt twelve experiments, the upright boiled gage was brought down to three-tenths of an inch above the ſurface of the mercury in the ciſtern, as it was in experiments the ſixteenth and ſeventeenth. And by both theſe laſt gages it ſeems very plain, that the vapour, &c. had a conſiderable effect at a much leſs degree of exhauſtion than what I uſed with the box-wood, by which I found no effect. Perhaps Mr. Nairne's box-wood was not ſo dry as mine; and I did not try it green, where the effect might have plainly appeared. Now all theſe experiments ſeem fully to prove what has already been obſerved, namely, that this coating, or covering with damp, &c. takes place moſt where the air is highly rarefied. [See page 149.] And in all theſe experiments the degree of exhauſtion is taken by the pear-gage in an erect poſition, of which notice has already been given. [See page 120.] Theſe experiments alſo further prove, that a common conſtructed air pump may be made to rarify as much as one of the Smeatonian conſtruction.

Mr. Nairne tells Mr. Bewly, November 22, 1776, that the pump with which theſe experiments were made, is different from Mr. Smeaton's, 'and as a proof that it is not liable 'to be out of order, I need only mention that 'it has been worked almoſt daily and hourly 'ſince the 21ſt of October till this day, and 'has not had a ſingle drop of oil more than 'what was put to it when it was firſt put 'together, and at preſent I do not find but 'what it performs all its functions as well as

‘ at firſt*.’ Hence thoſe experiments apparently were begun with this pump, at about October 21, 1776, but I knew nothing of what had happened any where, except what I myſelf was doing, till the 7th preceeding: ſo that I think my diſcoveries, relative to the fallacy of the Smeatonian pump and gage, ſtand quite independent of every thing elſe, any where, or at any time; and that all Mr. Nairne’s experiments, except what was ſhewn in April 1776, were poſterior to my diſcoveries relative to the pump and gage. But the real exiſtence of the vapour, by boiling the pear-gages, I did not diſcover till afterwards.

It appears that many Gentlemen of the Royal Society were preſent with Mr. Nairne, when he ſhewed Mr. Smeaton the great difference between the two gages, &c. in April 1776, [ſee page 134] one of whom was the

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* A ſtrong proof of the excellency of the manner in which the workmanſhip was executed.

Hon. Mr. Cavendiſh, whoſe great philoſophical knowledge is well eſtabliſhed. He then recollecting ſome former obſervations, very readily ſuggeſted an *hypotheſis*, whereby to account for the difference between the two gages which he had juſt before ſeen. The ſubſtance of this *hypotheſis* is given by Mr. Nairne in the Philoſophical Tranſactions, but ſomething different from the manner in which Mr. Smeaton gave it in his anſwer to my firſt letter to Dr. Watſon, as will appear by and by.

On the third experiment in Mr. Nairne's accounts [ſee page 136,] a note is given, which ſays, 'It may be proper to take notice, that the 'pump, in every experiment hereafter-men'tioned, was worked ten minutes, except where 'it is otherwiſe mentioned.' By this note it may appear, as if ten minutes were thought full long enough to work the pump. Hence it ſeems he had not tried what might be done by working the pump any longer time, or even ſuppoſed it neceſſary with the pear-gage; though it appears as if he did with the barometer gage. And I ſhould in general have thought the ſame, for I do not remember that I worked my pump five minutes, nor above three, my receiver being ſmall, [ſee page 116,] for any one of theſe experiments; and yet, I apparently produced as great a degree of rarefaction as any one. But in the ſucceeding experiments his ingenuity led him to ſee what would happen by working the pump half the time, that is, five minutes, and then puſhing down one gage, purpoſely having two of them at the ſame time in the receiver; but ſtayed till the pump had been worked ten minutes before the other was puſhed down, in order to ſee the difference produced by the difference of the time of working the pump. Thus when the firſt gage was puſhed down in this experiment, no more air was ſuppoſed to get out of this gage; although the pump was kept working to ten minutes. Accordingly the firſt gage indicated but a low degree of exhauſtion, to what it did in the former experiments, when the pump had been worked ten minutes. This difference, one would think, might have pointed out the trial of working the pump with a pear-gage, as much longer than common, as this laſt experiment was ſhorter; in order to ſee what difference that time of working the pump would make. I think it could have done no harm: and in particular, after what Mr. Nairne had repreſented to Mr. Bewly, December 19, 1776, when referring to the elaſtic vapour, (the idea of which the former informed the latter, he had from Mr. Cavendiſh) he ſays, 'The manner I account for theſe appearances is this: I 'imagine that there is contained in the leather, '&c. an elaſtic fluid which is kept in a fixed, or 'condenſed ſtate by the preſſure of the at'moſphere, but when that preſſure is in part 'taken from it, it turns into an elaſtic vapour, 'mixes with the dilated air and is exhauſted 'with it out of the receiver. That when the 'pump has been kept continually exhauſting 'for ten minutes, there then remains in the 'receiver and Smeatonian gage only a four 'thouſandth part of air, the other being the

‘ elaſtic vapour, which returns to its original ‘ fluid ſtate on being preſſed by the atmoſ‘ phere; in the ſame manner as ſteam when ‘ turned into water, which, if I remember ‘ right, takes up thirteen thouſand times more ‘ ſpace when in ſteam than in water, and I do ‘ imagine the longer the pump is kept exhauſt‘ ing that leſs air would be left in the Smea‘ tonian gage.’ However, this ſeems to have eſcaped all his careful variety of examinations; yet he might try it, and not finding it to anſwer, omit to inſert it. But he found the barometer, or upright gage, the ſame at five minutes working, as it was at ten. If the pear-gage was not thus tried, perhaps what prevented it might be the diſcouragement given by Mr. Cavendiſh, that is, ‘ that ‘ being continually generating and condenſing ‘ would never be exhauſted,’ as related in Mr. Smeaton’s anſwer to my firſt letter, on this ſubject, to Dr. Watſon, which, as I received it, appears as follows: ‘ Seeing theſe ‘ experiments, at which were preſent Mr. ‘ Cavendiſh, Mr. Aubert, and ſome other

friends,

‘ friends, and its turning out as Mr. Nairne ‘ had ſaid, Mr. Cavendiſh ſuggeſted, that ‘ under a certain degree of abſence, of preſſure, ‘ of atmoſpheric air, that the moiſture in, and ‘ about the pump and glaſſes, raiſed a vapour ‘ from the watery particles that being conti‘nually generating and condenſing would ‘ never be exhauſted, and which preſſed upon ‘ the open leg of the ſyphon and raiſed the ‘ column in the other juſt as much as a light ‘ preſſure of air; but as this watery vapour ‘ would even promote the eſcape of the at‘moſpheric air from the receiver, whenever ‘ the return of the atmoſphere drove the mer‘cury up into the pear-gage, the watery va‘pour condenſing to the ſide of the tube, left ‘ nothing but the proportion of atmoſpheric ‘ air, that under theſe circumſtances remained ‘ to be unoccupied by the quick-ſilver; in con‘firmation of this idea, he deſired that the ‘ experiment might be tried, that all moiſture ‘ might be avoided as much as poſſible; and ‘ as I remember, the ſyphon gage came much ‘ nearer to the level: and, as I *believe*, the '*p ear-gage* ſhewed a leſs rarefaction. But I 'was the leſs exact, in noting the particulars, 'as I intimated to Mr. Cavendiſh my deſire 'that he would give the ſociety an account 'of the experiment with his explication upon 'it, which appeared to me equally new and 'curious, therefore, in hopes that Mr. Cavendiſh would do it I reſted the matter 'there.'

The former being Mr. Smeaton's exhibition of Mr. Cavendiſh's hypotheſis, that in the Philoſophical Tranſactions now follows. After ſeeing the gages differ ſo much from one another, Mr. Nairne ſays, 'Mr. Cavendiſh 'accounted for it in the following manner. 'It appeared, he ſaid, from ſome experiments 'of his father's, Lord Charles Cavendiſh, that 'water, whenever the preſſure of the atmoſphere on it is diminiſhed to a certain degree, is immediately turned into vapour, 'and is as immediately turned back again 'into water on reſtoring the preſſure. This 'degree of preſſure is different according to

‘ the heat of the water: when the heat is 72°
‘ of Fahrenheit’s ſcale, it turns into vapour
‘ as ſoon as the preſſure is no greater than that
‘ of three quarters of an inch of quickſilver,
‘ or about one-fortieth of the uſual preſſure
‘ of the atmoſphere; but when the heat is
‘ only 41°, the preſſure muſt be reduced to
‘ that of a quarter of an inch of quickſilver
‘ before the water turns into vapour. It is
‘ true, that water expoſed to the open air will
‘ evaporate at any heat, and with any preſſure
‘ of the atmoſphere; but that evaporation is
‘ entirely owing to the action of the air upon
‘ it: whereas the evaporation here ſpoken of
‘ is performed without any aſſiſtance from the
‘ air. Hence it follows, that when the receiver
‘ is exhauſted to the abovementioned degree,
‘ the moiſture adhering to the different parts
‘ of the machine will turn into vapour and
‘ ſupply the place of the air, which is conti-
‘ nually drawn away by the working of the
‘ pump, ſo that the fluid in the pear-gage, as
‘ well as that in the receiver, will conſiſt in
‘ good meaſure of vapour. Now letting the

‘ air into the receiver, all the vapour within ‘ the pear-gage will be reduced to water, and ‘ only the real air will remain uncondenſed; ‘ cónſequently the pear-gage ſhews only how ‘ much real air is left in the receiver, and not ‘ how much the preſſure or ſpring of the ‘ included fluid is diminiſhed, whereas the ‘ common gages ſhew how much the preſſure ‘ of the included fluid is diminiſhed, and ‘ that equally, whether it conſiſts of air or ‘ of vapour.

‘ Mr. Cavendiſh having explained ſo ſatis-‘ factorily the cauſe of the diſagreement be-‘ tween the two gages, I conſidered, that, if ‘ I were to avoid moiſture as much as poſ-‘ ſible, the two gages ſhould nearly agree: ‘ this induced me to make the following ex-‘ periments.’

Hence it appears as if Mr. Nairne did not recollect Mr. Cavendiſh's deſiring all moiſture to be avoided, and the experiment to be tried, by ſaying, ‘ I conſidered that if I were to avoid moiſture,' &c. any more than Mr. Sméaton did Mr. Cavendiſh's deſcription of the neceſſary heat of water to evaporate in an exhauſted receiver.

The idea of the elaſtic vapour ſeems to be allowed on all ſides, as Mr. Cavendiſh's own: and were it not for the principle on which the idea itſelf is founded carrying with it ſuch an inſurmountable obſtacle, the idea would not be ſo eaſily ſhaken; but as it is reſted upon a ſuppoſition that on the admiſſion of common air to it, or on the admiſſion of it to common air, the effect being the ſame in both, it becomes of no conſequence, or returns to its original ſtate; thus the hypotheſis ſeems wholly to deſtroy itſelf. He ſays, ' but ' as this watery vapour would even promote ' the eſcape of the atmoſpheric air from the ' receiver,' &c. or as Mr. Nairne relates, in the Philoſophical Tranſactions, ' that water, ' whenever the preſſure of the atmoſphere on ' it is diminiſhed to a certain degree, is immediately turned into vapour, and is as immediately

‘ mediately turned back again into water on ‘ reſtoring the preſſure: or, that, it is continually drawn away by the working of the ‘ pump, or, as he ſaid to Mr. Bewly, that ‘ it mixes with the dilated air and is exhauſted ‘ with it.’ On theſe premiſes, it ſeems almoſt impoſſible that this vapour ſhould by any means promote the eſcape of the atmoſpheric air: for if it ſhould be able to aſſiſt the rarefied air to lift up any valve, (and in particular, one whoſe reſiſtance is equal to Mr. Smeaton’s) in the middle part of it, yet when it gets to the limits of its confinement, that is, at the edges of the valve, it is inſtantly driven back whence it came: ſeemingly as if the common air ſtood to watch it with a ſure weapon, ready to demoliſh it at its attempting to make its eſcape. Or it might be repreſented more familiarly, by ſuppoſing a ſhower of rain and hail falling upon a large plate of glaſs, or the like, on which the ſhower would fall, rain and hail mixed together; but the hail would by the fall be made to bolt back, or up again; and although the water might lie lurking ready to paſs, yet it could never get through the glaſs, any more than either the dilated air, or the vapour under the receiver, could eſcape at the valve.

In Mr. Cavendiſh's hypotheſis, he ſays, as related by Mr. Smeaton, 'the watery vapour 'condenſing to the ſide of the tube left nothing but the proportion of atmoſpheric air, 'that under theſe circumſtances remained to 'be unoccupied by the quickſilver.' I think he muſt conſider the vapour to condenſe, and lodge perfectly ſmooth, contrary to the nature of vapour, damp, &c. on the inner ſurface of the tube, &c. and to leave no lodgements, or cavities, for any of the air to lurk in, as the mercury riſes in the gage. But there is not a ſingle hint in the hypotheſis, or the experiments, towards making the vapour appear, in any ſhape, to any of the ſenſes; and it is much to be doubted whether any of either vapour, or air, enters the tube more than what is perpendicularly under the column contained in the tube-part of the gage, agreeably to the rules of hydroſtatics, namely, that fluids preſs equally in all directions: whereas the boiled gages make it plainly appear; and alſo, if they are examined with a magnifier, that there are innumerable cavities, &c. as already obſerved, for the air to lurk in, all the way up in the bulb, be it ever ſo large, as well as the tube of the gage: and moſtly the larger the bulb is, and the ſmaller and ſhorter the tube is, the greater is the deception, if the unoccupied part of the gage is taken in an erect poſition. But Mr. Nairne does not mention how he took the unoccupied part of his gages, whether in an erect, or horizontal poſition; which makes a very great difference. [See page 120.]

In the Philoſophical Tranſactions, page 623, it is ſaid: ‘ Now letting the air into the re-‘ ceiver, all the vapour within the pear-gage ‘ will be reduced to water, and only the real ‘ air will remain uncondenſed; conſequently, ‘ the pear-gage ſhews only how much real air

‘ is left in the receiver*.’ But the pear-gage no more ſhews how much real air is left in the receiver, than what is left in the gage itſelf; on account of the lodgments and cavities, formed by the damp or vapour, on the ſides of the glaſſes, &c. nor do thoſe experiments make it appear that the vapour is reduced to water.

Mr. Nairne ſays, ‘ The other being the ‘ elaſtic vapour, which returns to its original ‘ fluid ſtate on being preſſed by the atmoſphere; ‘ in the ſame manner as ſteam when turned into ‘ water,’ &c. [See pages 161 and 162.] But this is ſomething contrary to moſt of the other conceptions of this vapour; which ſuppoſe that it is changed immediately into water, or its original ſtate: whereas ſteam ſeems to remain ſome conſiderable time before it is perfectly turned into water, or back again into its original ſtate.

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* The ſame is maintained alſo in Philoſophical Tranſactions, pages 636 and 637.

On the firſt of May, 1777, Mr. Bewly wrote to me as follows: ‘ In a letter which ‘ I received this morning from Mr. Nairne, ‘ whom I had informed of ſome of the par-‘ ticulars of your experiments, he ſays, there ‘ is certainly ſomething or another that Mr. ‘ Brook does not attend to; otherwiſe there ‘ would not be that difference in his gages:’ and then gives ſome examples with his gages, adding, that ‘ they always agree, I never find ‘ my gages differ. He ſuſpects too, as I did, ‘ that you have not attended to the *time*. The ‘ time of working the pump is certainly a very ‘ material conſideration; if the hypotheſis of ‘ an elaſtic vapour be true.’ In reading Mr. Nairne’s account in the Philoſophical Tranſactions, I was very glad to ſee, that he afterwards found a difference in his gages, as well as I had done in mine*. And in reſpect to attending to time, all my guide was the bringing the boiled upright gage as low as I could; which I believe never exceeded three minutes, and often not ſo long; and yet my apparent powers, or degrees, or number of times rareſying, were equal to any, as mentioned juſt before: ſo that the *time* does not appear to be of ſo material a conſideration as was believed, whether the truth of the hypotheſis be admitted or not.

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* See Philoſophical Tranſactions, pages 626 and 627.

Having uſed my pump indifferently with wet leathers, &c. and having informed Mr. Bewly, that the effect went off in ſix or eight hours, and ſometimes much ſooner, by working it often in the time; and ſending accounts of ſome experiments, to him, made with the pump in that ſtate, and from the intercourſe between himſelf and Mr. Nairne, the latter adviſed the former not to uſe any water or leathers ſoaked in water, for he muſt never expect his pump to exhauſt after he had uſed water, any thing like ſo near as it did before. This being contrary to what I had told Mr. Bewly, it added much to my ſatisfaction in reading Mr. Nairne's account in the Philoſophical Tranſactions, to find, that my former obſervations to Mr. Bewly were ſo ſtrongly verified*.

Mr. Nairne, having been deſcribing the conſtruction of his long and ſhort upright gages, ſays, 'gages made with theſe precau-'tions ſeem to me to be the moſt to be de-'pended upon, in determining the actual di-'minution of the preſſure on the ſurface of 'the quickſilver in the ciſtern†." And it may be readily ſuppoſed, that this ſort of gages will be the moſt to be depended upon in every pneumatical operation relative to rarefaction or condenſation. For the pear-gages ſhew nothing at all till after the deſigned exhauſtion is over; whereas the other ſorts do it continually from the leaſt to the greateſt degree: and even after the deſigned exhauſtion is over, the pear-gage ſhews no more than the other would under the ſame circumſtances, that is, by being boiled, namely, that there is a vapour, &c. that ariſes and takes place on the ſides of whatever is within the receiver, at a conſiderable diminution of atmoſpherical preſſure: and this verifies that part of Mr. Cavendiſh's hypotheſis in a very high degree.

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* See Philoſophical Tranſactions, 642. † Ib. page 620.

After finding Mr. Nairne's compariſon of the two pumps inſerted in the Philoſophical Tranſactions; as I knew him to be as great a ſtranger to ſecrets as Dr. Prieſtley is to perſiſting in a miſtake; the next time I went to London I took one of my boiled upright gages with me, intending to aſk his leave to let me try one of his beſt Smeatonian pumps with my gage, which he ſeemed with great pleaſure to grant: and he having juſt before finiſhed a new pump on that principle, with ſome additions and improvements, he readily gave me leave to try what I liked with it, and deſired his foreman to aſſiſt me. But when we had put my gage on his pump, and exhauſted it, he ſeemed to be ſomething diſappointed, to find that his pump would not bring my gage any thing like ſo low as he expected. We repeated our trials, but the reſult was the ſame: and I ſhould have been equally diſappointed had his caſe been mine; though he ſaid but little about it: nor could we with that pump, which was finiſhed in the beſt manner, bring my gage ſo low as I could with theſe pumps which I myſelf had made, by full one-tenth of an inch: and I could only ſuppoſe it to be owing to the difference of boiling the gages: which, to me, ſoon appeared very plainly to be the caſe; though Mr. Nairne had conceived by verbal deſcription that my method of boiling them could not be ſo perfect as his, on account of the poſition in which I held my gages, while they were boiling. Accordingly he deſired his boiling apparatus to be got ready, in order to let me ſee his method of boiling; which was done: and I could do no leſs than offer to ſhew him my method, which was likewiſe done one night after we had been at the meeting of the Royal Society, where he was ſo obliging as to aſk me to go with him. And as I was boiling a barometer tube, he ſeemed ſatisfied that my method of boiling was not inferior to his, in regard to perfection*; though I found that he could boil ſeveral to my one: but it appeared to me that his method was ſcarcely boiling at all, or little more than heating them pretty hot: nor did it appear to me that he could boil a peargage at all. Some little time after my return home I received a letter from Mr. Nairne, in which he informed me, that they had diſcovered that ſome of the ſhoulders of the pump which I tried at his houſe, were not ſcrewed ſo cloſe as they ſhould have been; which was the reaſon why the pump would not exhauſt any further. I gave him for anſwer, that if the ſhoulders were not ſcrewed cloſe, the pump would not have been air-tight; but in that reſpect it appeared to be very perfect; therefore that could not be the

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[*] Notwithſtanding all this, the next time I ſaw Mr. Nairne, he had, amidſt his multiplicity of buſineſs, quite forgot my boiling a barometer tube for him, though ſome of his workmen, who were preſent, remembered it very well: alſo he did not remember my ſending him a boiled gage for an air pump, from Norwich, which I had alſo done.

reaſon why it did not exhauſt any further. Here ſeems to be a more full demonſtration, that the common conſtructed pump may be made to exhauſt equal to the Smeatonian one.

From the experiments related in Mr. Nairne's accounts in the Philoſophical Tranſactions, which appear to have been conducted with a great deal of ingenuity and care, together with thoſe herein related, I think it plainly appears, that the ſuppoſition relative to the excellency of the Smeatonian conſtructed air pump, aroſe entirely from the fallacioufneſs of the pear-gage; and that the common conſtructed pump, as well executed as Mr. Nairne does his, will exhauſt as much as any hitherto conſtructed*.

Some time towards the latter end of April 1777, I received a letter, dated only *midnight*, from Mr. Bewly, incloſed with a latin pamphlet, which pamphlet was ſent hither to aſk the aſſiſtance of Dr. Manning, in a phraſe or two, which the Doctor readily gave him in a note concerning the pamphlet, with a poſtſcript; and ſent it to me to forward to Mr. Bewly; which I did May 10, 1777. The following is the ſubſtance of the poſtſcript. 'Would 'Dr. Anybody, or Mr. Anybody, at London, 'or any where, have ſuppoſed that the Smea'tonian pump would do no more than any 'other good pump; which was Mr. Brook's 'firſt diſcovery; and alſo that the Smeatonian 'gage was not an accurate one, or at leaſt, that 'it varied ſo much from the other, and that it 'alſo varied ſo much in itſelf at different times, 'before his experiments led the way?' but Mr. Bewly gave no anſwer to the poſtſcript, although he wrote to me various times very ſoon: on which Dr. Manning gave me another note to encloſe in a letter which I ſent to Mr. Bewly, June 2, 1777. I tranſcribed the note, and the following is a copy of it. 'Dr. 'Manning's compliments to Mr. Brook, and

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* Mr. Nairne informed me that he has lately conſtructed an air pump ſo as to work without a valve in order to exhauſt further, but I do not remember that he told me how much farther than his former pumps it would exhauſt.

‘ thanks him for the peruſal of Mr. Bewly's
‘ letter, he is much diſpoſed to be pleaſed
‘ with any thing of his, as he has formed a
‘ good opinion of both his head and heart;
‘ but pray inform him that he does not ſeem
‘ to apprehend the meaning of his queſton
‘ added to your former letter, I will therefore
‘ intimate what it meant, namely, in plain Engliſh,
‘ that the whole courſe of experiments
‘ relative to the two gages and the preſent
‘ compariſon of the common pump and that
‘ of Mr. Smeaton's, took their riſe from your
‘ experiments communicated to Mr. Bewly
‘ and Dr. Watſon.’ So that it ſeems to me, that I am not the only one who thinks, if any claim is to be laid to the diſcovery of the fallacy of either pump or gage, that I have the greateſt.

As obſerved before, Mr. Smeaton deſcribes his improvements, if they were ſuch, on the air pump, in the XLVIIth Volume of the Philoſophical Tranſactions; and he deſcribes two advantages in one paragraph, page 420, which

runs thus: ‘ Another advantage of this con-
‘ ſtruction is, that though the pump is com-
‘ poſed of a ſingle barrel, yet the preſſure of
‘ the outward air being taken off by the up-
‘ per plate, the piſton is worked with more
‘ eaſe than the common pumps with two
‘ barrels: and not only ſo, but when a con-
‘ ſiderable degree of rarefaction is deſired, it
‘ will do it quicker; for the terms of the ſeries
‘ expreſſing the quantity of air taken away
‘ at each ſtroke do not diminiſh ſo faſt, as
‘ the ſeries anſwering to the common one.

The former of theſe two advantages is attended with two notes; and ſeems to want more, to enable it to keep firm on its baſis: the former is as follows:

‘ It is obvious that theſe improvements
‘ will equally obtain, whether the pump is
‘ conſtructed with a ſingle or double barrel.’
And the latter ſtands thus:

‘ Becauſe, though the preſſure of a column of air, equal to the diameter of the
‘ piſton rod, ſtill preſſes upon it, yet, as there is
‘ only the friction of one piſton, and that not
‘ loaded with the weight of the atmoſphere, the
‘ friction of the leather againſt the ſide of the
‘ barrel, and that of the rack and wheel, is much
‘ leſs: ſo that notwithſtanding the addition of
‘ friction in the collar of leathers, that of the
‘ whole will be leſs.’

In the double barrelled pump, the piſtons are connected together by the racks and wheel; therefore, whatever is lifted up by one piſton is preſſed down by the other, excluſive of the friction; and though there is the friction of the leathers in both barrels, his has likewiſe the friction of the piſton againſt the barrel, and the friction of the rod in the collar of leathers: and though the rod is leſs in diameter than either of the piſtons in the double barrelled pump; yet the Smeatonian piſton is much larger, otherwiſe one of the ſuppoſed advantages is loſt. And although it is ſaid that *the piſton is worked with more eaſe than the common pumps with two barrels;* yet I cannot find how it can work with ſo little eaſe, if they are well executed: but perhaps Mr. Smeaton here means by *common pumps,* thoſe of an inferior ſort of workmanſhip; if not, then I think the principle is much in behalf of the common pumps.

The other advantage, which is, to rarefy quicker, is left without any aſſiſtance; therefore, is almoſt ready to fall of itſelf. For with two barrels, their ſize need not be over large, to be equal in capacity to the Smeatonian ſingle barrel, whatever they may exceed it. And as the wheel in the latter is uſually made ſmaller, and the winch longer, in order to make the labour leſs, than in the former, and the barrel conſiderably longer; conſequently the hand has to paſs through a much greater ſpace. I think that, upon an average, four dips, or ſtrokes, with the two piſtons, may be made, in the ſame time, or perhaps ſooner, than *one,* in the Smeatonian conſtruction. And if one barrel in the double conſtruction ſhould take away only half the quantity of air at each dip of each piſton, that the ſingle barrelled conſtruction will do; then if only two ſtrokes (namely, one ſtroke with each piſton) with the former, can be made in the ſame time that one ſtroke can be made with the latter, I think the quickneſs of exhauſting is nearly equal; or if a greater number of ſtrokes can be made with the former, in the ſame time, than one in the latter, I think the common pumps, like that which I ſaw, which came from Mr. Nairne, particularly with a ſmall alteration* in the piſton, are very much preferable. Conſequently this deſtroys what is advanced in the ſecond note.

The firſt note ſays, 'it is obvious,' &c. Therefore of what uſe is the note, as it illuſtrates nothing? for the improvements, if they

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[*] This alteration I thihk I could plainly ſhew to any body, particularly to a workman: but I cannot well do it in writing, without a copper-plate.

were ſuch, which the note refers to, ſeem to be deſcribed in a preceeding paragraph which is as follows: ‘ Hence, as the piſton may ‘ be made to fit as near to the top of the ‘ cylinder, as it can to the bottom; the air ‘ may be rarefied as much above the piſton as ‘ it could before have been in the receiver.’ But the improvement referred to ſeems to be of no uſe, namely, a method to aſſiſt the valve to riſe eaſier. For I believe it will readily be allowed, that, be the conſtruction what it will, there muſt always be air enough left to raiſe the valve; alſo, that this will be the caſe no longer than a more perfect vacuum can be made in the barrel below the piſton, than there is in the receiver on the pump, be their capacities little or great: and alſo, that this will be the caſe, whether, or not, the piſton works in a partial vacuum; for as the piſton in a common pump can be made to fit at the bottom as cloſe as it does in a ſingle barrel pump; (and I think they may with care be either of them made to fit as cloſe as any other ſubſtances, or ſurfaces, even ſo as to exclude all air between them, as to ſenſe) then, whether the barrel works in a partial vacuum, or not, the ſpace under the piſton will be as perfect a vacuum, as the materials will admit; unleſs any air can get into that ſpace out of the receiver: which will depend chiefly on the eaſe with which the valves can be made to open and ſhut, and the Smeatonian upper valve is equally expoſed to the preſſure of the common air, as the upper valves in a common double barrelled pump. Therefore the ſuppoſed uſe of the note, as well as the improvement it refers to, does not clearly appear. Nevertheleſs, the plauſibility, with which the whole is related, is excellent, and wants no aſſiſtance whatever. Probably, this brought on that ſtrength of expreſſion by Mr. Bewly. [ſee page 127.] However, excluſive of all theſe plauſibilities, and ſuppoſed excellencies, the principle itſelf, on which they are built, runs diametrically oppoſite to that moſt perfect and unerring rule in mechanics, as well as throughout all philoſophy, that whatever is gained in power is loſt in time.

CHAPTER V.

OBSERVATIONS ON THE BAROMETER.

WHOEVER obſerves the Barometer attentively, with reſpect to the alterations of the weather, relative to wet and dry, will find, that though its indications are not always certain, yet on the whole it is moſtly ſo, or, that it is the beſt director we have. The theory of it being very well underſtood, I ſhall omit it, and chiefly attend to the manner of making that inſtrument.

A variety of incidents following each other brought on my making barometers. It already appears what occaſioned the diſcovery of the fallacy of the Smeatonian pump and gage: one circumſtance was the enquiry of various perſons about them, one of whom was Mr. J. Ferguſon, F. R. S. who, as already obſerved, gave a courſe of lectures in Norwich, in the year 1769, and the luminous flaſk in his lecture on electricity was the firſt I ever ſaw; and though I had no air pump to do it as he did, he told me how to make one without a pump; which was, by joining a tube to the mouth of a flaſk, then filling both with quickſilver, and inverting them in the Torricellian way, and ſealing the mouth of the flaſk before I ſeparated them. My method of joining the tube to the flaſk was by the help of cement: but by the warmth of cementing the flaſk and tube together, previous to their being filled, I found much damp in the flaſk when I had ſeparated it from the tube. I afterward tried to diſperſe the aforeſaid damp in the flaſk, by warming it before ſeparation; but found no advantage. This put me upon trying a thin glaſs tube in the Torricellian method;

thod; but, when it was filled with mercury, I found it was not ſo clean as it ſhould have been; or it appeared as if wet withinſide: and having but a ſmall ſtock of tubes, I was the more anxious about preſerving them: but by having Mr. Beighton's account of Mr. Orme's manner of managing them (which was alſo merely incidental as will ſoon appear, and of which notice will be taken in the ſequel) I tried to diſperſe the moiſture by heating the tube with the mercury in it before a common fire; which ſoon turned the moiſture into large bubbles of air: thus I was encouraged to try to get them out of the tube: but they chiefly vaniſhed when they came where the tube and mercury were cold.

My next apparent reſource was, firſt, to drive away all the moiſture from the ſealed end of the tube, and then to make its whole length paſs the heat in ſucceſſion: which anſwered very well, and left the mercury very bright in the tube.

Finding the advantage of this laſt method ſo plainly to appear, a more convenient method to heat them ſoon occurred, and was eaſily put in practice. Thus, by the time that I had done three or four tubes, I found that I made them as perfect as I could make them at all. But though this method aſſiſted me conſiderably afterward, I was much diſappointed in my preſent purſuit, namely, of making the exhauſted flaſk for electricity: for let the tube be of any length it would above the mercury as a barometer, I could not make any electrical light appear in the void. Soon after this, I finiſhed my air pump, already mentioned; with which I reduced the column of mercury to half an inch: but the effect was the ſame. So that I got no aſſiſtance from heating, for making luminous flaſks: nor did I now want it. And as the aforeſaid method of heating is what I ſtill prefer, I ſhall briefly deſcribe it. I procured two ſheets of tin from the tin-plate workers (iron is better, as will appear by and by) bent to a right angle in the middle of it the longeſt way, and riveted together, one at the end of the other. A partition of the ſame is fixed about ten inches from one end, with a part cut out from its upper ſide to within a little more than an inch of its bottom, and about three-fourths of an inch wide. The whole is to be placed on a ſtand of a convenient height, on a declivity, making an angle of about ten degrees with the horizon, with that end uppermoſt towards which the aforeſaid partition is fixed: then above this partition, lay ſome baker's coals (in preference to charcoal) cloſe to the partition, in order to be kindled; of which two or three handfuls are ſufficient for one tube.

After this preparation, take a glaſs tube not leſs than three feet long, with a bore about three or four twentieths of an inch in diameter, but not more (as will appear afterward) and the tube to be nearly as thick on all ſides as the diameter of the bore, that it may be ſufficiently ſtrong. Let this tube be nicely ſealed, or cloſed, at one end, and let it be as clean as poſſible: then fill it with mercury as pure as poſſible; when the coals are kindled in the caldron, or fire-holder, lay the ſealed end of the tube, with the mercury in it, into the place cut out of the partition, upon the lighted coals: if the fire be ſlack, blow it a little with the mouth, or the like; let the tube lie amongſt the lighted coals, till the mercury in it boil pretty ſtrongly. When the firſt part of the tube appears to be ſufficiently boiled, move it onward, by little and little, and ſo on the whole length of the tube.

After the tube is filled in the common way, before it is put upon the fire, take out about three inches of the mercury at its open end, in order for room, that the mercury may play up and down as it boils, without being loſt. Thus, by taking theſe methods, I find no inconvenience ariſing from the heat of the tube in holding it whilſt it is boiling; for, by the time that the ſealed end has paſſed the fire, before the other comes to be ſo near it as to be too hot to be held in the hand, the former will be cold enough to hold the tube by, while the remaining part of it goes on. [See Monthly Review, Vol. XLIX, page 584.]

Tubes for barometers prepared in this way, I find generally, will ſupport a column of mercury of the ſame height: or, that inſtead of a variation of half an inch, as ſometimes happens with thoſe made in the common way, the former will very ſeldom vary half a tenth of an inch. Alſo, if they are well boiled, the mercury will always be ſuſpended to the top of the tube, and will not fall down to its proper height, till it be ſhaken pretty ſtrongly to bring it down; and ſometimes I have had them ſo ſtrongly ſuſpended, that after the barometer has been ſent many miles, it has been returned as uſeleſs. One inſtance of this kind happened at the time I was writing this article.

The ſuſpenſion of the mercury to the top of the tube occurred in the very firſt tube that I heated before the common fire, though the adheſion was weak; but the more perfect ſuſpenſion did not occur till afterward; which ſeems to be owing wholly to a more perfect excluſion of the air, damp, &c. from within the tube: whence the adheſion appears to be ſtronger between the glaſs and the mercury, than it is between the particles of mercury themſelves; for the mercury will frequently ſeparate at an inch or more from the top of the tube, and fall down to its proper height, while the upper part of the column will remain ſuſpended where it was. Then, by inclining the tube, the mercury will join, and ſometimes only a part of the column that remained will come down, and a ſhorter part of it will ſtill remain ſuſpended; which is brought down likewiſe by inclining the tube; and ſometimes this will happen months after the barometer has been finiſhed and fixed up.

When the tubes are completely boiled, they may be hung up with their ſealed end downwards, till they are wanted, when they may be cut off to their proper length, with a file, in which I never exceed thirty-two inches and a quarter, (preferring the upright barometer) and then they may be fitted up in whatever manner may be thought moſt proper, either for ornament as well as uſe, or for uſe only, either in a portable way or not. But if they are to be fitted up in a portable way, I always preſerve a hole through which the air may circulate into the ciſtern, which is opened or ſhut by means of a ſcrew, and is equally good as if it were open all round.

Not long before the time that the foregoing obſervations occurred, it happened, that I, profeſſionally, purchaſed a collection of books, &c. amongſt which was No. 448, of the Philoſophical Tranſactions, which contains the account of Mr. Orme's improvements of the barometer, given by Henry Beighton, F. R. S. which ſtands as follows.

‘ Mr. Charles Orme’s improvements of the ‘ barometer, by the method following:

‘ Firſt, the *quickſilver* is all purified from ‘ its droſs and earthy particles by diſtillation; ‘ and when the *tube* is filled by a pound and ‘ half, two or three pounds of *mercury*, and ‘ all the *air* got out by the methods uſed in ‘ filling *tubes*, then the remaining *air* is got out ‘ by ſuch an *intenſe heat of fire* as makes the ‘ *mercury boil;* by which ebullition an innu-‘ merable quantity of ſmall particles are emit-‘ ted, and blow with a great velocity at the ‘ open end of the *tube*, till all the *air* is quite ‘ cleared out; which curious as well as fa-‘ tiguing operation is continued for the ſpace ‘ of four hours: and when no more bubbles ‘ would riſe in the *tube*, it remained whole, ‘ with its *mercury* of a moſt lively ſparkling ‘ brightneſs, with this difference only, that ‘ the *mercury*, ſo purged from its air, did not ‘ *fill* the *tube* ſo high as when firſt put in by ‘ about two *inches;* which is a plain demon-‘ ſtration, that in that *tube*, which was 49

‘ *inches long*, there was interſperſed in the mercury at firſt filling it, ſo much *air* as would ‘ fill two inches of the ſaid *tube*, which was a ‘ 24th part of the ſaid ſpace.

‘ The whole operation I myſelf attended the ‘ 20th of January, 1734-5.

‘ And further I can affirm, that every part ‘ of the *mercury* boiled for a long time, and ‘ the *tube* was *gradatim* ſo *red hot*, that with a ‘ warm knife I could make impreſſions in any ‘ part of it.

‘ And this I the rather mention, by reaſon ‘ I have heard ſeveral perſons, and thoſe not ‘ incurious, affirm it was impoſſible.

‘ And that this is the moſt ſure and certain ‘ (if not the only) method for getting out all ‘ the *air*, may be judged by the boiling of ‘ water, which in its ebullition does emit a ‘ great quantity of *air* for a long ſpace of ‘ time.’

We are none of us at preſent, that I know of, ſo fortunate as to be maſter of the art of bending glaſs tubes, or making impreſſions on them, when they are filled with mercury, by any means whatever. I have often, in vain, wiſhed ſo to do: which makes me ready to think, that Mr. Beighton muſt, ſome way or other, have been as much miſtaken in this, as Dr. Prieſtley found himſelf to be, relative to the Smeatonian pump or gage, already obſerved. And I muſt own that I ſhould have been much more ready to believe it, were it not, that all my own obſervations in boiling tubes or the like, directly contradict the fact. I will here relate a caſe which happened before I had made myſelf ſo much acquainted with boiling tubes, &c. as I was afterwards.

Having formed a plan to make a barometer in an inverted poſition, in order to get it more out of the way of accidents, I made a bulb ſufficient to hold about a pound of mercury, at the end of a barometer tube of a ſufficient length, to be turned up at the lower end, ſyphon-wiſe, that the riſe and fall of the barometer might appear in the turned-up part, which inverts the ſcale; and the bulb at the top to anſwer the end of a ciſtern. I have always found barometers made in this way to ſupport the longeſt column between their upper and lower ſurfaces: and as the fact is a ſufficient proof, I omit the theory at preſent.

I ſay, I have found barometers made in this way to ſupport the longeſt column, when boiled; but boiling them in this ſhape was not ſo eaſy at firſt. When the bulb and its tube were filled with mercury, in order to be boiled, I put the bulb in a hollow place amidſt the coals, with the barometer in an erect poſition, in order that the bubbles might riſe into the tube, and afterward be driven out by boiling in my common poſition. But, in my haſte, I did not conſider the preſſure of the whole column of mercury on the bulb, and that it would retard the appearance of its boiling; ſo that I heated it very much, even ſo hot, that the bulb was ſoftened enough to give way: but the mercury being ſo much preſſed and heated, as ſoon as the glaſs gave way, the mercury contained in the bulb inſtantly turned into vapour and diſperſed entirely, and only what was in the tube ran down into and through the fire, which was very fleet. So that I cannot conclude upon Mr. Beighton's deſcription of Mr. Orme's method any otherwiſe, than that he muſt have been very much miſtaken, or knew very little about it. Beſides, whenever I diſtill mercury, I always do it in a glaſs veſſel, but never find the glaſs to be at all ſoftened: and when I boil the mercury in the tubes, either thick or thin, they appear to have no ſoftneſs or tendency to bend in the leaſt.

And I muſt own, that I never found any ſuch diminution in the length of the mercury in the tube after boiling, as is above deſcribed; provided that I took care to loſe none of it out of the tube while it was boiling

ing: which makes me think, that there muſt have been ſomewhat elſe beſides the exploſion of the air that the mercury contained, which was the cauſe of the tube not being filled ſo high as when the mercury was firſt put in by about two inches, or a twenty-fourth part of the ſpace; nor did I ever find any *blowing* at all at the open end of the tubes, though I have occaſionally boiled tubes much ſmaller and much larger (as will appear very ſoon) than Mr. Orme's appear to be, wherever they are ſeen: ſo that I cannot think the demonſtration of either is plain enough to be depended upon.

Mr. Beighton, ſpeaking of the boiling, &c. ſays, 'which curious as well as fatiguing operation is continued for the ſpace of four hours: 'and when no more bubbles would riſe in the 'tube it remained whole,' &c. I am ſure that if the heat were continued four hours more, if there were mercury enough remaining unevaporated in the tube, the riſing of the bubbles would likewiſe be continued: my tubes never take up more time, to boil one of them, than three-fourths of an hour, and often not ſo much. Therefore I muſt conclude, that if Mr. Beighton had been as much acquainted with the boiling of mercury in tubes of glaſs, or the operations carried on in a laboratory, as he was with thoſe carried on in the clouds and the atmoſphere, he would never have written his paper in the manner it now appears.

Amongſt the aforeſaid collection of books, &c. was a pamphlet written by E. Saul, A. M. which in many parts is very curious and exact, but not altogether ſo throughout. In this work, pages 51 and 95, Mr. Saul recommends the ſloping barometer. In page 101, for an upright barometer, he recommends a tube of nearly half an inch diameter (it muſt be ſuppoſed that he means the bore of the tube) in order to judge of the convexity or concavity of the ſurface of the mercury in the tube occaſioned by the alteration of the preſſure of the atmoſphere on the mercury in the ciſtern;

and that the mercury will riſe and fall ſooner than in a ſmaller bore; Mr. Saul here ſays, ‘ if the *mercury* appears to be concave, or de- ‘ preſſed more in the middle, than at the ‘ ſides of the tube,’ &c. But it is certain that if a tube be clean withinſide, the mercury will never appear concave on the ſurface, be the tube great or ſmall, and in particular at its ſides. Let any common *clean* phial, of any ſize whatever, be partly filled with pure mercury; the ſides will always appear convex and the middle not depreſſed, even if a ſyphon be applied in the middle of it to decant it out of the phial: all the time that the ſurface of the mercury is ſinking, its ſides will have a convex appearance; but if the phial be moiſt, or the mercury foul, the ſurface of the latter will not be the ſame: it is only where purity is wanting, that concavity takes its place with reſpect to mercury itſelf in theſe caſes.

At the end of the XLVIIth Volume of the Monthly Review is the annunciation of that almoſt ineſtimable work of Mr. De

Luc's on the barometer, &c. which was about a year after I had acquired the method to boil tubes for barometers*, and I cannot but own that the manner of the finiſhing of the account in the Review concerning it rather prejudiced me againſt it. However, when the book came to me, I read it with as much pleaſure as I ever had in reading any book whatever: and although he knew no more of what I was doing, than I knew of what he was; if I had put down my own obſervations and deſcription of the appearances and proceſs of boiling tubes for barometers, they would have been as near to his as if we had had the moſt familiar intercourſe.

The preference that is to be given to tubes whoſe bores are not very large and their ſides pretty thick, ariſes chiefly from the danger that attends them in carriage or on any removal: beſides, a tube of about two lines bore I find to be much eaſier, and ſooner boiled, than one

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[*] See the note in Monthly Review, Vol. XLIX, page 585.

one whoſe bore is three or four lines in diameter: and the apparent ſpecks of air are more eaſily kept from being left behind. But in tubes whoſe bores are large and their ſides and tops thin, the motion of the mercury at any time very much endangers them. Mr. Nairne, knowing my method of boiling barometers, &c. ſent two tubes, with large bores and thin ſides, ready filled to me to boil for him; which I did, and returned them as ſafely packed as I could: but before he received them they were both broken to many pieces; conſequently all the mercury as well as the tubes were loſt and ſpoiled. But I never knew one ſuch as I have already deſcribed, to break in carriage. When tubes are *well* boiled, they ſeem to me to be much more liable to be broken by the motion of the mercury, than tubes that have been but *moderately*, or not at all boiled. I ſay, they ſeem to me to be much more liable to be broken, for this reaſon, that in unboiled tubes, the lining, that Mr. De Luc very juſtly ſpeaks of, greatly ſaves them, and ſoftens the ſtroke of the mercury againſt the glaſs: for the ſtroke is manifeſtly ſmarter in tubes that have been well boiled, than it is in tubes that have not been boiled at all; owing to the air, or that lining, which nothing will diſplace but heat, being all diſperſed, and the glaſs being left quite bare and naked: ſo that the ſtroke of the mercury meets with no reſiſtance but exerts its force immediately on the glaſs; and the glaſs has no defence againſt the mercury.

Thoſe apparent ſpecks of air, juſt mentioned, though in boiling they have that appearance, yet they ſeem to be nothing but mercury in an evaporated ſtate, or the vapour of mercury not perfectly condenſed, till the tube grows cool; when they quite diſappear: yet I do not like to leave them behind. When the mercury is boiling in the tube pretty ſtrongly, there will often be a ſpace of an inch or two, or more, in tubes, whoſe bore is rather ſmall; which ſpace at the ſame time is perfectly filled with vapour from the mercury; and appears, while hot, extremely elaſtic, and diaphanous as air itſelf: and, if care be taken, when the fire is rather low, the upper ſide of the bore will ſoon become ſtudded with particles of mercury condenſed upon it: but while the vapour remains uncondenſed, its elaſticity appears to be very great. Mercury, when boiling, ſeems to turn much faſter into vapour, than water when boiling into ſteam; and likewiſe turns very quickly into mercury again, when in a cool place, if care be taken to preſerve it from eſcaping.

The thickneſs of the glaſs has been ſaid to hazard the breaking of the tube at the time it is boiling. But if the bore be not leſs than one eighth of an inch, they may be very ſafely boiled, if the fire be ſmall and clear: but if the fire be large or long, their preſervation is very hazardous. A tube one eighth of an inch in the bore may very ſafely be boiled, let the thickneſs of the glaſs, moderately ſpeaking, be what it will. A tube half an inch diameter divided into three parts, two for the thickneſs of the glaſs on each ſide, and one for the bore, is as good as any, in my opinion.

If a tube break whilſt it is boiling, great part of the mercury is liable to be much damaged, by amalgamating with the tin on the plates in which the fire is laid: but if, inſtead of ſheets of tin, an iron one is at hand, the amalgamation is all avoided: yet if the mercury be fouled in this or any other way, by having tin, lead, or the like mixed with it; it is eaſily reſtored to its original purity by diſtillation, be their quantities what they will: but if the proportion of adulteration be rather ſmall, the mercury is ſtill more eaſily made as perfectly pure by agitation; a method diſcovered by that indefatigable philoſopher Dr. Prieſtley; which method was much improved by Mr. Bewly, and afterward much more improved by the Rev. Mr. Morgan; and by which it is now become very uſeful*.

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* The method I uſe for this purpoſe is, to boil the adulterated mercury in a common four ounce phial for the ſpace of about a minute,

Whatever may be the caſe in general with barometers, thoſe made after the preceding method (two of which I have had by me more than ten years) do not appear to be the leaſt impaired for a long ſpace of time, but ſuſtain the ſame length of column as in one newly boiled: ſo that by this, as well as by the air pump, pure mercury does not ap-

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minute, in which time very little, or none, of the included mercury will evaporate out of the phial, or leſs time will do if the whole of the mercury boil perfectly in the phial, over ſome lighted coals from a baker's oven, being leſs offenſive than charcoal; but a common kitchen fire will do as well, if it be not too fierce: then take it from the fire, and when it is cool, ſhake or agitate the mercury in the phial pretty ſtrongly: and if the quantity of adulteration be not very great, a quarter of a pound of the adulterated mercury ſo boiled, and briſkly agitated in the phial, will become perfectly pure in leſs than a quarter of an hour: but if the adulteration is very much, or if the quantity adulterated be a pound or more, in the former caſe the operation will ſucceed much better, by mixing ſome pure mercury unboiled with that which is not pure and has been boiled; and then churning or agitating them together: or in the latter, it will do quicker, by pouring about a quarter of a pound of it at a time, into another phial, and churning it in that, and ſo on till the whole is purified, putting the purified portions all together in a ſeparate phial: when the fouled pear to imbibe atmoſpheric air, or but in an extremely ſmall degree, and very different to what Mr. Beighton has repreſented it, if the veſſel be clean in which the pure mercury is contained: but if the veſſel be foul, the air which the foulneſs contains will make itſelf appear on the air pump; yet it cannot be ſaid to come out of the mercury. One of the aforeſaid

mercury has been churned ſome time, inſtead of appearing nearly enough done, it looks much more foul, and the purification comes on almoſt all at once as to appearance; which may ſeem ſingular: and if the quantity of fouled mercury be a pound or two, a beginner in this way of purification may be diſcouraged by its not going on ſo faſt as might be wiſhed. In this caſe, divide the fouled mercury into ſmaller portions, as before obſerved. When the fouled mercury has been ſufficiently agitated, and is become pure, the adulteration will turn black and adhere to the ſides of the phial, or ſwim on the ſurface of the purified mercury; and they will not eaſily mix together again: then pour the purified mercury through a funnel made of common writing paper, with a ſmall orifice at the bottom: the purified mercury will run through the funnel, and leave all the blackneſs or adulteration in the funnel, where it may be ſqueezed by the fingers; and force the remaining part of the mercury through the funnel: but the foulneſs will all remain behind, ſo that very little of the mercury need be waſted throughout the operation.

aforeſaid barometers has very often been placed on an air pump; and the column of mercury in it brought down to within leſs than half an inch of the level with the mercury in its ciſtern, and as often let up again: ſtill the mercury apparently ſtrikes the top of the tube as ſmartly, as if it had never been brought down at all: and taking the whole time of the aforeſaid, *more than ten years* paſt, the columns of mercury in theſe barometers have been, for more than two-thirds of the time, not leſs than thirty inches high between the ſurface of the mercury in the ciſterns, and the ſurface at the tops of the columns in the tubes, and often-times very much higher, yet I have ſeen them down to 28, 35: and 30, 82 was the higheſt I ever ſaw them.

FINIS.

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